THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[THIRD SERIES.]

ART. LII.—*On Southern New England during the melting of the Great Glacier;* by JAMES D. DANA. No. IV.*

IV. DEPRESSION OF THE LAND, OR AMOUNT OF SUBSEQUENT ELEVATION.

THE higher of the terraces of stratified drift, especially of those along the shores of estuaries and tidal inlets, or within the lower tidal part of the river valleys, are rightly regarded as evidence, *after making the necessary deductions*, of the amount of upward change of level which the region containing them has undergone. The following are some heights above high-water or flood level observed in Southern New England:

1. The terrace plain, or upper level of the stratified drift, in the northeast corner of the New Haven plain, near Whitneyville —50 feet; and in the northwest corner, at Westville—47 feet.

2. On the Housatonic River near Birmingham, ten miles from the Sound and nine miles west of New Haven—95 feet.

3. In the Connecticut valley, in the vicinity of Middletown, about thirty miles from the Sound and twenty-three miles northeast of New Haven—150 feet.

4. At the head of the fiord called the River Thames, at Norwich, sixteen miles from the Sound and nearly fifty miles north of east of New Haven—110 feet.

5. At the head of Narragansett Bay, about Providence, a mile above the mouth of Providence River, and about 100 miles north of east of New Haven—80 feet.

Here we have terraces at five places in Southern New England, thirty miles or less from the Sound, and within reach of

* For the preceding parts of this memoir see pp. 168, 280, 353, of this volume.

the tides, having the following heights above flood-level: near New Haven, 47 to 50 feet; at Birmingham, 95 feet; near Middletown, 150 feet; at Norwich, 110 feet; and at Providence, 80 feet. What are *the necessary deductions* to be made from these numbers in order to obtain for each place the amount of that change of level (the depression of the land when the deposits were made, and the subsequent elevation) which led to the present elevated position of these plains of stratified drift, making them into river-valley and estuary terraces? If any elevated shell-bearing sea-beaches existed on the shores of the Sound, the question as to the amount of elevation since the Champlain period would be easily answered. But such evidence as already explained, is wanting; and wanting also outside of the limits of the Sound, even to Cape Cod.

In order to make correct observations on this subject and arrive at right conclusions, several points have to be noted, a brief review of which is here given by way of introduction.

1. The height to be measured should be that of the highest terrace plain above the level of the highest modern flood-plain or lower flats. For the terrace-plain, where of full height, marks approximately the flood-level of the Champlain era, just as the flood-plain of a stream in these modern times marks the level nearly of modern floods. This point has been considered in obtaining the numbers for the heights above given, each being the height of the terrace-plain above flood-water level.

2. An allowance has to be made for the excess in height of the Glacial flood—that attending the melting—over the height of modern floods. If modern floods raise a stream 20 feet above low water mark, and hence make a flood-plain at that height, and a Glacial flood were so great as to produce a rise of 50 feet above the same low-water line, the flood-plain made from the depositions of such a Glacial flood, if at the water-level, would be 30 feet higher than that from ordinary floods; and when the flood subsided, the flood-plain so made would exist as an elevated terrace, 30 feet above the lower flats, although there had been no change of level whatever. The terrace here is evidence of the height of the flood, and not of any change of level in the land. Moreover, supposing in addition, an elevation to have taken place, 30 feet of the height of the Glacial flood-plain, if of the full normal height, would have to be credited to the excess in height of the Glacial flood.

Moreover, the deposits of those thirty feet may all have been made in *shallow* water, that is, each near flood-level, as the flood rose in height; so that the marks of shallow-water origin in the lower and overlying beds would not in this case be proof of a gradual subsidence as the deposition went forward.

3. A further allowance has often to be made on account of

local obstacles to the free discharge of the waters, giving a local height to the flood, much exceeding that usual along the valley. When a river valley is suddenly contracted, or when large tributaries join it at a given point, the stream may be carried in such places to an abnormal height—and so produce deposits up to that abnormal level—because the water cannot escape as fast as it is supplied. Moreover, for the same reason, the depth of water in the receiving estuary, and in the Sound just off the mouth of the stream, are elements to be considered; since, if very shallow, the water, under a flood of supposable magnitude, might be raised in an estuary several yards above the true tide-level; and a few feet perhaps over shoals off its mouth in the Sound, notwithstanding the wide spreading of the water on either side.

4. To guard against error, the true limit between the *stratified* drift of a valley or estuary and the *unstratified* of the adjoining hills must be carefully determined, for only the former is the terrace or flood-made formation. To be sure of correctness on this point, it has to be considered that the *unstratified* drift has sometimes locally a degree of stratification of a tumultuous kind, due to the rush of waters from the melting glacier; and, again, the *stratified* drift may fail of distinct bedding. Hence in all cases a wide range of level terrace-plain—so wide that there can be no reasonable doubt that it was levelled under water, should be sought for, in order thereby to confirm the evidence from other sources as to the deposits being of under-water origin.

An important mark of such river-valley, estuary or sea-border formations consists in the total or nearly total absence of great bowlders from their level surface. The slopes of the hills above carry bowlders usually down to the level of the plain; then they cease suddenly, scarcely one existing over the plain that tops the water-made deposits. This may be observed along all the river valleys of New England, and also in many places on its shores. Between Watch Hill and Point Judith, the hills are thickly strewn with the huge blocks, down to within eleven feet of high water level; there, along a large part of that coast, wide meadows are spread out without a bowlder. But wherever the surface over the meadows swells upward above the eleven-foot level, it is peppered as thickly with bowlders as the slopes of the neighboring hills. Some bowlder-areas occur that are scarcely above it because the surface there was at the water's level when the bowlders were dropped. The ice, as melting went forward, let go first the great rocks that were in its bottom or lower part, and where there was water underneath instead of land they sunk and were spread, along with much coarse gravel, over the bottom, there to be buried beneath later depositions. The material of these later beds along the river

valleys was brought down and deposited by the flooded river as the melting went forward and the glacier retreated; or they were carried into the valley from the hills either side of the valley—all the slopes under the deluge of waters sending down earth and stones, as well as the ravines and side-valleys, to be stratified and levelled off by the rushing stream.

It is not safe to infer that all New England hills with grassy bowlderless surfaces are parts of a valley terrace; for some regions afforded few bowlders to the ice; or only soft easily decomposable material; but when the hills rise to a common height and have a flat top, there can be little reason for doubt.

5. It is further to be noted that the terrace or terraces of a region may be only one or more of the *lower* terraces instead of the highest; that is, in other words, the *highest* normal to the region may be wanting, either (1) because there the flood-waters made no depositions up to flood-level, owing to its fierce rate of flow, or some other cause, but built up only to lower levels; or (2) because the highest part of the formation, after its deposition, was swept off and so reduced to a lower level by the flood when in its period of greatest violence, leaving only a lower terrace in its place, with perhaps traces of the higher, or often, not even these.

6. A careful study of the kind of *stratification* in the deposits is needed in order to determine whether the beds were deposited (1) under the action of the *incoming tide* on the sands and gravel from the dissolving glacier or drift-covered hills; or (2) under the action of the *outflowing river*. This study requires that the cuttings for all road-gradings, wells, sewers, and cellars, in the stratified drift of an estuary, should be carefully examined. When the facts as to structure are fully understood, the question whether *sea-made* or *river-made* may in this way receive a positive answer even along shores having no raised shell-bearing sea-beaches.

7. An examination should be made for stratified deposits of the *later* or "Alluvian" part of the Champlain period, and a comparison of the heights of such beds with that of the *early* Champlain beds or true "Diluvian" stratified drift; for such beds may show at what level the waters of an estuary or inlet stood after the Glacial flood had for the most part or wholly subsided.

The mere statement of these points is sufficient to evince that the problem before us—that of determining the amount of depression of Southern New England during the Champlain period, or, of elevation since then—is beset with difficulties. These difficulties I have not been able in all respects to surmount; and my present purpose is to state the facts which I have observed, and not to announce positive results. The ob-

servations needed to settle all doubtful points demand long-continued investigation in each estuary region of the coast, such a study as can be satisfactorily carried forward only by one who is living in the region and is thus always at hand to take advantage of every excavation that may be made in the deposits for cellars or other purposes. And to this should be added an accurate topographical and hydrographic survey of the estuary regions, and of the river valleys within thirty miles of the coast.

After these preliminary considerations I proceed to the discussion of the facts bearing on the level of Southern New England in the Champlain period, or, its equivalent, the amount of elevation which may be proved, from existing conditions, to have taken place since that period.

I. New Haven Region.

The evidence with regard to the amount of depression over the New Haven region which has been obtained is from the following sources:

1. From the height of the upper or highest terrace of a region above mean high tide.
2. From the depth of the valley-excavations along the rivers and estuaries, or the height of the upper terrace above the flood-plain of the present streams.
3. From the structure of the beds.
4. From the existence of "Alluvian" deposits.

1. *Height of the terrace-plain or stratified-drift formations above mean high tide.*—This terrace-plain in the New Haven region is the wide plain upon which the city is built. (The map of the region is here reproduced, in order that the relations of the localities referred to beyond may be appreciated). It extends *southward*, by the west side of the New Haven Bay, through West Haven to the Sound east of Savin Rock—a rocky point terminating the first range of hills on the west. It is also continued northward through Hamden to Mount Carmel; but it gradually diminishes in width in this direction and at last is confined to the vicinity of Mill River. The distance from Savin Rock to Mount Carmel in an air-line is eleven and a half miles, and it is one sloping plain. From the upper part of the city (about four miles north of Savin Rock), where the land is 45 to 47 feet above mean high tide, there is a gradual slope seaward (that is, southward), toward the West Haven coast, of 10 to 11 feet a mile; and there is also the same gradual rise northward toward Mount Carmel. No trace of any beach-like break is to be found across the surface; no line of sand hills or ridges; nothing whatever to make one part more than another to look like the course of a beach or water-line.

This is well seen, for the lower part of the plain, when standing on a bridge in the West River valley: an unbroken terrace makes the sides of the river valley from Westville, where the height is 47 feet above mean high tide, to the mouth of the stream, where, opposite Oyster Point, it is 20 feet; and going on toward the West Haven beach, the plain continues its slope, terminating with a height of 5 feet at the beach, or 8 feet just back near the western hills. And for the middle and upper part of the plain it is apparent along the road (Dixwell avenue) passing over the great plain from New Haven northward into Hamden, where the surface over a very wide area is all open to view so that any abrupt change of level or variation in the surface would be easily detected if such existed. There are hence on the surface of the plain no marks of a shore-line of the Champlain period.

Again, no sea-shore or beach deposit is found anywhere about the lower slopes of the hills above the level of the plain. The passage is every where abrupt from the even plain to the rising hill-side, that is, from the terrace of stratified drift to the unstratified drift of higher levels, and this is true quite to Savin Rock on the Sound. On the east side of this bay the same is true; but the terrace on that side is mostly wanting, and the cape is rocky, so that the fact is less significant.

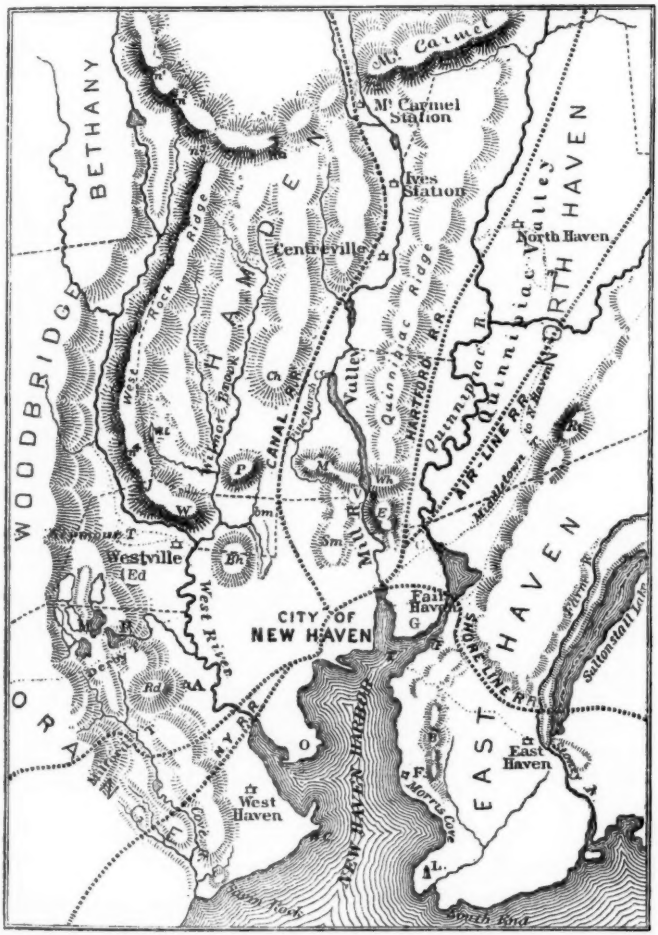
The following are the elevations of the plain at different points between the Sound and Mt. Carmel village.

					Feet.
Near Savin Rock beach, on the Sound,					5
0·5 mile north of Sound, highest near foot of hills,					10–14
1·5 miles	"	"	S.W. of bridge over West R.,		18
2	"	"	c. Kimberly and Greenwich sts.,		23
2·5	"	"	on Putnam st.,		31
2·9	"	"	c. of Congress av. and Cedar st.,		35
3·5	"	"	c. of Elm and York sts.,		42
4	"	"	c. of Dixwell av. and Henry st.,		49
4·5	"	"	c. of Dixwell av. and Division st.,		52
5·3	"	"	c. of Dixwell av. and Dudley st.,		61
6	"	"	Dixwell, near house of H. Munson,		68
6·5	"	"	Hamden church on Dixwell av.,		76
7·25	"	"	Dixwell av., near J. E. Bassett's,		82
9·5	"	"	N. Centreville, at meeting of roads,		103
11·5	"	"	near Mt. Carmel village,		115

An even slope is not to be expected since (1) the shore flats or flood-grounds of a river or lake are never even; and because (2) terraces have often lost much of their height by denudation, or gained or lost in height through drifting sands.*

* I am indebted to Mr. C. E. Fowler, New Haven City Surveyor, for the heights of the various parts of the plain along the streets within the limits of the city; and to levelings by Mr. G. B. Chittenden and D. H. Pierpont, carried on under the direction of Mr. R. M. Bache of the Coast Survey, for heights through Hamden to Mt. Carmel.

MAP OF THE NEW HAVEN REGION.



Explanations of the map.—A, Allingtown village. B, Beacon Hill. Bh, Beaver Hills. Ch, Cherry Hill. C, Air Line Railroad cut, near south end of East Rock. E, East Rock range, consisting of East Rock proper to the northwest, Indian Head, and then Snake Rock. Ed, Edgewood, the estate of Donald G. Mitchell, Esq. F, Fort Hale. *F*, Ferry Point, or Red Rock, on the Quinnipiac. J, Judges' Cave, on the West Rock ridge. L, Light House. M, Mill Rock. M P, Maltby Park, only three of the proposed lakes of which are constructed. O, Oyster Point. P, Pine Rock. Rd, Round Hill. Rt, Rabbit or Peter's Rock. Sm, Sachem's ridge. T, Turnpike; also Tomlinson's bridge, across the head of New Haven bay. V, Whitneyville. W, West Rock, the south end of the West Rock ridge. W C, West Cape, or West Haven Point. Wh, Whitney Peak. W L, Wintergreen Lake, just north of Wintergreen Falls. Wn, Warner's Rock. *bm*, Beaver Pond Meadows; *m*, Mineral Spring, southeast of North Haven; *n1*, *n2*, *n3*, *n4*, different notches in the West Rock ridge; *n1*, *n2*, the upper and lower Bethany Notches; *n3*, the Hamden Notch; *n4*, the Wintergreen Notch.

Scale 4-10ths of an inch to the mile.

The part of the plain stretching northward through Hamden to Mount Carmel village, reaching there a height of about 115 feet above the sea, owes its slope to the pitch of the flooded stream, which there flowed over a sandstone region with no deep channel to confine its waters and direct their course. That it was not due to marine submergence to a depth of 115 feet is proved by the facts in the adjoining Quinnipiac valley, a mile and a half to the eastward. For at North Haven, six miles north of New Haven, the stratified drift or terrace has a height above flood level not exceeding 45 feet; and this is but two feet above its level five miles to the south, just north of west of New Haven city; facts which show that there was a nearly level surface over the great Quinnipiac basin or harbor between these distant points, and that the greatest height of the marine submergence, if there was one, could not have exceeded 45 feet.

If then the upper part of the sloping plain from Mount Carmel southward was made by fresh waters from the melting glacier, shall we conclude that this was true also of the lower part, nearly to the West Haven shore? The slope of this lower part is no greater than that more to the northward; and hence there is little objection to the conclusion from this source. The bay is small and throughout shallow, being but 3 to 18 feet deep at low tide, and little of it over 12 feet; so that it did not afford a capacious channel for the escape of the flood waters.

2. *Height of the terrace-plain above flood-level in the estuary and river-valleys.*—About the New Haven bay, the plain, as has been stated, rises gradually to the northward. It reaches its greatest height above the lower flats or flood-plain of the streams near where they leave their valleys to pass through the plain.

A. *West River.*–The greatest depth of the cut along West River, or greatest height of the terrace-plain above the lower flats, exists near Westville, where it is 45 to 47 feet. Going up West River this height above flood-level diminishes to 33 feet in the course of a mile, and to 6 to 8 feet in the next half a mile; but this is due to the rapid descent in the stream, which is 22½ feet in the first mile, and 50 feet in the following half a mile, 72½ feet in all.

B. *Mill River.*—Along Mill River, the height of the terrace for the first mile and a quarter above the head of New Haven bay, that is, through the New Haven plain, is ten feet or so below the height normal to that part of the plain; and this was a consequence of the denudation by the Glacial flood, as before explained.* But at Whitneyville, near the dam, the height—or the depth of the cut—is 50 feet; and this continues to be the

* This volume, page 177.

depth up stream for the next mile and a half; and for the next three-fourths of a mile, or up to Augurville, the height is 45 feet. Toward Mt. Carmel village the height diminishes to 36 feet above flood level; and then in the next half mile above to 25 feet.*

C. *Quinnipiac River.*—The terrace plain along the Quinnipiac near where it leaves its valley, or at the Air Line Railroad cut, is 43 feet. North of this lie the wide marshy flats, which continue for five miles to North Haven; and at the latter place the height of the terrace on the west of the river is but 45 feet; the waters having stood nearly on a level between the northern and southern limits of this great interior basin.

These are the facts from the river-valleys. Along each the cut made by the denuding waters since the Glacial flood is not far from 45 feet. This depth of cut is just what would at the present time result from the denuding action of the rivers, if the land were to be raised 45 feet or so—supposing there were no rocky ledges in the beds of the streams to prevent excavation downward.

On the other hand, if the Glacial flood were vast enough to raise the waters to that level and so to build up (out of the sands and gravel within its disposal) the drift formation there existing, without any aid from an elevation of the land, or of one of only 5 to 10 feet, then, when the flood had passed, the streams would have sunk their beds to their present level, and so have left the terrace-plain, where not more or less washed away, at its present height above them.

Thus either hypothesis, (1) that of an elevation of 45 feet since the Champlain period, or (2) that of the accumulation of stratified drift in the valleys to its present height by the Glacial flood, with little or no subsequent elevation of the land, will explain the facts. If the former were the actual condition, then the waters would have continued their work, about the estuary or inlet, of building Champlain deposits up to, or toward the 45 foot level, through all the Champlain period, or until the elevation of the land closing the period began. If the latter, the sinking of the rivers as the flood subsided would have lowered their working ground down to the existing level of those plains; and hence, little or no estuary deposits of the later or "Alluvian" part of the Champlain period, beyond a height of 5 to 10 feet above high tide level, should be looked for.

For further evidence we proceed now to facts from the *structure* of the beds of stratified drift, and from the occurrence of

* This diminution in the depth of the cut, or in the height of the terrace-plain, to only 15 feet, is owing to a trap ledge which crosses the bed of the stream in a line with the trap ridge at Mt. Carmel; this ledge prevented excavation.

deposits of the later or "Alluvian" part of the Champlain period.

3. *Structure of the beds formed.*—Throughout the stratified drift-deposits of the New Haven region for four and a half miles north of Savin Rock on the West Haven coast, the finer beds of the stratified drift are characterized by the flow-and-plunge structure; and the oblique lamination in it—certain river valley regions excepted—*rises to the northward* (that is, dips to the southward). This *rising to the northward* I have attributed, in a former part of this memoir, to movement from the southward in the waters, that is, to plunging waves accompanying the *inflowing* tides—supposing that ordinary inflowing tidal waters may push up the sands of the bottom before them and so make ordinary obliquely-laminated layers dipping seaward or rising landward: and that, by the addition of plunging waves, the flow-and-plunge structure produced would also have the layers dipping seaward. That in such a case the dip of the little layer would be seaward I inferred, perhaps without due consideration, from the direction of movement in the water, and from observations long since made by me on the sandstones in the vicinity of Sydney, New South Wales.* If, as assumed in Part I of this memoir, the above conclusion is correct, the seaward dip of the oblique laminæ beneath the New Haven plain, up to a height of at least 45 feet, is proof that the sea was the agent, and that therefore the land at the time was at least 45 feet below its present level.†

Further, the deposits through the long Air-Line Railroad cut at the mouth of the Quinnipiac valley, have, as described on page 175 of this volume, the oblique lamination in the upper stratum of 20 feet *reversed*; that is, dipping landward instead of seaward; and this difference in the lower and upper strata naturally suggests that there was a change in the direction of movement of the depositing waters; a change from that of the inflowing tide to that of the outflowing flood, as stated on page 176; or, if the lower stratum was made by the flood, from the outflowing flood to the inflowing tide. Either way we reach the conclusion that, at some time in the early Champlain period, if not through all of it, the land was at least 45 feet below its present level, and that a rise to this extent has since taken place.

Still certain facts dispose me now to question the inferences that oblique lamination made by the inflowing tide would have the dip in general seaward, and that the reversion of the dip

* See the writer's Exploring Expedition Report, 4to, 1849, pp. 465, 523. The observations were made in 1839.

† Owing to the want of good sections I have been unable to study the character of the lamination over the Hamden Plains, where the height of the surface rises beyond 45 feet.

in the upper twenty-foot stratum on the Quinnipiac has no other explanation than a change in the direction of the current. Among them, I here mention only that I have observed several cases along river-valleys, out of the reach of the sea, of dip in both directions; and, in some of them, obliquely laminated layers dipping down stream had a thickness of several feet. Such facts bear against the view.*

4. *Alluvian Deposits*.—Another fact favorable seemingly to the conclusion that the land remained 40 feet or more below its present level for some time after the Glacial flood had passed, is afforded by the existence of the deposit of horizontally-bedded white sands against the northward slope of the terrace of stratified drift at the northeast extremity of the Air Line Railroad cut, near south of East Rock. The figure is here reproduced

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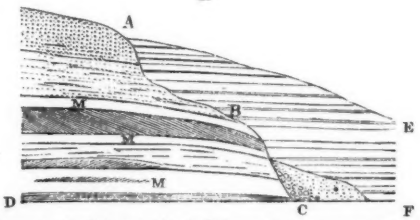


Section of stratified drift, A C D, and later unconformable Champlain deposits, A C F E, Air Line Railroad cut.

that the relation of the deposit to the stratified drift may be appreciated. The conclusion, explained on p. 180, that the white sands were washed and accumulated on the south shore of a great "Interior basin" occupying the Quinnipiac valley, appears to be the only reasonable one; and if so, this "Interior basin" continued to exist for a while after the flood and flood-depositions had reached their height, as an area of high water. Yet, as the bed is of very limited extent and the terrace formation is generally free from Alluvian deposits, the time may have been only that of the latter part of the flood, prolonged after flood-deposition had ceased by the melting still going on to the north; and the region of melting may have been north of the head of the New Haven streams, since it is possible, as I show beyond, that the flood waters of the Connecticut and Farmington rivers overflowed into the New Haven region and bay.

5. *Conclusion*.—While the height of the terrace-plain above the sea and its height above the flood-plain of the river, fail of demonstrating that the New Haven region was below its pres-

* The gentle pushing of the waters over sands makes ripple-marks, or elevations having a seaward and a landward slope. The query now is whether, in the heavy pushing producing the oblique lamination, the laminæ would conform to the former or the latter of these slopes.

ent level in the Champlain period, the evidence from the structure of the beds and from the Alluvian deposits suggests that the region was submerged to a depth of at least 45 feet.

But if so deeply submerged, why should the terrace plain slope off gradually toward the coast and have no line of old beaches across it? And why is there no 45-foot sea-shore formation or terrace along some part of the bases of the hills?

The ways in which such a slope in the plain may be supposed to have resulted are the following:—(1) By a subsidence causing a bending downward of the coast-region after the elevation of 45 feet had taken place.—(2) By an elevation of the land which should be 45 feet four miles from the coast, but 10 to 12 feet near the coast.—(3) By the sweeping action of the flood giving a slope to the deposits toward the Sound and beneath its waters, and preventing the formation of seashore flats at the water's level.—(4) By the denuding action of the waves and tidal currents as the land rose to its present level.—(5) By the deposits, because of a diminution in the amount of material southward or increase in the depth of the waters, conforming in slope to the pitch of the surface on which they were spread out.

The first and second of these ways have little probability in their favor. The others I make the chief source of the result in my memoir on the New Haven region, saying (p. 99) after remarks on each method: "That part of the rapidity of slope in the lower portion of the plain below 40 feet in height is due to tidal, wave and river action over the region of the bay; and part to increasing depth over the borders of the bay southward and a decrease southward in the amount of transported sand." How far these methods are sufficient may be better considered after a survey of the facts from other parts of Southern New England.

2. The Lower Part of the Housatonic Valley.

On page 409 I have stated that the height of the terrace of stratified drift in the Housatonic valley near Birmingham is 95 feet above high-water level. The terrace has the same height at Birmingham, and at the manufacturing village of Shelton on the opposite side of the river.* These places are situated at the junction of the Naugatuck river with the Housatonic. The Housatonic river comes down from the northwest, and the Naugatuck from the north; and on the point of land between

* For this altitude I am indebted to measurements by Mr. D. S. Brinsmade, of Birmingham, surveyor. The leveling was made at Shelton, and gave for the average height of the plain above low-water 106·64 feet. Mr. Brinsmade states that some points were five to eight feet above this. I have taken as the maximum height of the terrace 112 feet. The height of flood-level above low water seldom exceeds 17 feet; and this subtracted from 112 gives 95 feet for the height above flood-level. A higher flood, Mr. Brinsmade says, occurred in 1857, when the river rose to 22 feet 3 inches.

lies Birmingham, with Shelton opposite on the west (or southwest) bank of the Housatonic, and the village of Derby on the east bank of the Naugatuck. The streams rise in the high regions of Western Massachusetts and Connecticut, in part over 1000 feet above the sea, and flow in rapids through much of their course down to their confluence, where they reach tide-level, ten miles from the Sound.

As Birmingham is but nine miles to the westward of New Haven, it is improbable that the amount of elevation which the two places have undergone since the Champlain or Fluvial period should differ much; and hence there must be a special cause for the great height of the Housatonic terrace. For this cause we have only to look to the topographical conditions of the region.

Besides the fact of (1) the junction there of two rapid streams, whose united drainage area is about 1800 square miles, there are the following additional facts bearing on the subject:

(2) Both streams at that place have narrow terrace regions, the valleys between the hills having little width.—(3) On the east side of the Naugatuck the high bordering hills leave almost no space at their foot for the one-street village of Derby, so that the flooded waters cannot spread in that direction, and are naturally forced over against Birmingham and Shelton.—(4) The hills that lie back of Shelton, embaying its terrace region, reach to the river just below the village, and so do those on the Derby side, making thus, below Derby, a comparatively confined passage for the united waters, which has long been called the "Derby Narrows."—(5) The river channel below Derby, through its ten miles to the Sound is very shallow, being navigable for vessels drawing four feet of water only at high tide; and besides there are sandbanks at its mouth reaching a mile and a half beyond.—(6) The terrace along the valley below the Narrows is narrow for nearly seven miles out of the ten, the width in no part exceeding two-thirds of a mile.

Under such circumstances it is natural that the waters of the Glacial flood plunging down the two valleys should have entered the region of Birmingham and Shelton faster than they could have escaped. The fact that a cataract of great volume passed the region of Birmingham and dashed violently against the Shelton shore is abundantly manifested by the multitudes of large water-worn rocks and thick coarse cobble-stone deposits in the upper portion of the lower as well as high upper terrace, and in the imperfect stratification of portions of the deposits.

Even modern floods in the region bring down waters faster than they are discharged through the Derby Narrows and the channel beyond. The Housatonic river below the confluence

has almost no slope at high tide, the tide reaching that point. Yet a spring flood often produces a rise of 17 feet at Shelton, and causes an average pitch in the river's surface for the ten miles to the Sound of nearly $1\frac{8}{4}$ feet per mile; and there is occasionally a flood causing a rise of 22 feet, and consequently an average pitch of more than 2 feet a mile.

To ascertain the average pitch below Birmingham or Derby for the Glacial flood we have important data in the height of the upper terrace between these places and the Sound.

The Shelton and Birmingham level of 95 feet extends below Derby to the "Derby Narrows." Below the Narrows—about $1\frac{1}{2}$ miles from Shelton and 1 mile from Derby—the valley on its *east* side expands rather abruptly about the mouth of Two-mile Creek; it then contracts again on that side, through the rising of "Turkey Hill," but regains its expansion below through the retreat of the hills on the *west* side.

Now the terrace below the Narrows, just north of Two-mile Creek, has a height of 75 feet above high-water mark; and a mile below this, on the west side, of 70 feet. Hence, in passing the Derby Narrows, a distance of three-fourths of a mile, the level appears to have fallen 20 feet, which would indicate a tremendous rush of waters between the rocky hills.

The following table contains the heights of the terraces as measured along the valley to the Sound, the above included:*

			Feet.
At Shelton, opposite Derby			95
1 m. below	Derby,	east side, below the Narrows	75
2 m.	"	west side	70
4 m.	"	east side	50
5 m.	"	east side	46
6 m.	"	west side	43
6·8 m.	"	west side, near a stream	35
7·5 m.	"	½ m. above Railroad crossing	31
8·25 m.	"	near Stratford bridge	30
9·75 m.	"	½ m. from the Sound	12–20

The average decline in the height of the terrace from Birmingham, where, being 95 feet above flood-level, it is 100 above tide-level, to the Sound is over 10 feet a mile.† The rate of decline should not be uniform because it would vary with the varying width of the valley and depth of the channel. Moreover, in the case of most terraces, it is not certain that they were built up quite to the surface of the flood. The fact of a slope in the waters

* The heights given in this memoir, where not attributed to others, were obtained by the writer, and all of them with a hand-level, excepting four, above 120 feet, which were measured with a good aneroid.

† Five miles below Derby, west of the Housatonic, and one mile up its tributary, Farmill River, I found the height of the terrace 60 feet. This height was, however, that of the tributary, and not properly that of the Housatonic.

is, however, proved, and the general amount of pitch seaward deduced cannot be far from the truth.

For determining the actual amount of elevation along the lower Housatonic since the Champlain period no satisfactory data have been obtained. It is plain that the height of the terrace at Birmingham is a measure chiefly of the vastness of the flood, and that the "necessary deductions" for reducing it to a measure of elevation are very large. The evidence with reference to the elevation derivable from the structure of the beds could not be studied for want of sections. The wide extent of the thirty-foot terrace-plain suggests that it may mark the Champlain water-level; but of this other proof is needed. The formation of the seashore terrace would not require an elevation of more than 12 or 15 feet.

3. The Lower Part of the Connecticut River Valley.

The height of the stratified drift deposits in the Connecticut River valley at Middletown, 150 feet, seems, at first thought, to be evidence of a very great change of level since the era of deposition, much greater than in other parts of Southern New England. But what part of the rise in the waters was due to the great flood? The Connecticut River passes through the whole length of New England from north to south, and has a drainage area of about 20,000 miles. The melting glacier would have continued to augment its waters through its long retreat from the Sound to the borders of Canada.

First, a few words on the condition of the river supposing the Glacial flood to have raised the waters to the height mentioned; and, then, the facts which prove that such was the height and extent of the flood.

The Middletown region, 30 miles from the mouth of the river, with the floods at a height of 150 feet above the modern flats would have been in the condition of a lake one to four miles wide, and over part of the area, 100 to 150 feet deep. This Connecticut valley lake would have extended northward, narrowing and having shallow borders between Cromwell and Glastenbury (or between 3½ miles and 11 miles from Middletown), then widening again so as to cover an area a dozen miles in width about Hartford. I am informed by General T. G. Ellis, recently in charge of the Government Survey of the Connecticut River, that the stream at low-water at Hartford (15 miles north of Middletown) is on a level with mean low water in the ocean. Passing Hartford, it would have continued to Springfield and beyond; but it was probably in this part a broad rapid stream, with shallow borders most of the way on either side of its main channel, and having part of the pitch of the present stream, about 38½ feet in its course between

Springfield and Hartford, a distance of 24 miles. The whole width of the waters at Springfield was probably greater than at Hartford, although averaging much less—as the height of the terrace near the river shows—in depth. The Springfield level extended 10 miles to Holyoke, where are the South Hadley Falls of 60 feet.*

The evidence as to the reality of this height and of the great breadth of the waters is as follows:

Three-and-a-half miles north of Middletown, east of the river, a broad terrace rises boldly from the river to a height above flood-level of 144 feet,† and the wide plain continues, with a nearly even summit, though mostly of greater height 150 and 155 feet, to Glastenbury, eight miles to the north. The road from Middletown to Hartford on that side of the river, for the most of the way, is on the terrace. In addition to its level features, there is other proof that the formation beneath is the true stratified drift in its consisting, as shown in sections, of stratified sands and gravel. A terrace at very nearly the same height exists also for part of the way on the west side of the river in the town of Cromwell. The coarse nature of the beds comes from the rapid flow of the waters, and this from the little depth over the region where they were deposited—a fact made manifest by the sandstone rocks showing themselves occasionally through the plain. About Middletown, where the depth was great, the deposits consist of fine earth and clay.

Prof. Hitchcock, in his "Surface Geology," gives the height of the South Glastenbury "terraces" as 175 feet above the river, which would be about 152 feet above flood-level.‡

In the city of Middletown, the corresponding terrace is dis-

* Nearly 32 feet out of the 38½ of descent between Springfield and Hartford is made along Enfield Falls, beginning 8 miles below Springfield and continuing for 5 miles over a rocky bottom.

The height of the Holyoke dam at either end is 97·60 feet above low-water mark at Hartford, or mean low water in the ocean.

The tide up the Connecticut River is sometimes noticeable as far as Windsor Locks, at the lower limit of Enfield Falls, 10 miles north of Hartford. The highest in summer at Hartford, 45 miles from the mouth of the river, has a range of about 10 inches; and that at Middletown, of 1·93 feet.—*Gen. T. G. Ellis, in Reports U. S. Engineer Dept. for* 1868 *to* 1874.

† For the height of the terrace at this point I am indebted to a leveling by the following persons connected with the Sheffield Scientific School: Mr. G. W. Hawes, assistant in the Mineralogical department, and H. A. Miller, H. Hun, and A. B. Howe.

‡ Prof. Hitchcock suggests in his "Surface Geology" that the terrace at South Hadley of "292 feet" above the ocean, and that at Willimantic of "268 feet" slopes southerly, and corresponds with that of "200 feet" at Springfield and Long Meadow (136 feet above the river), that of East Windsor of "96 feet," and that of Hartford of "61 feet" above the ocean. Such a slope is impossible; and further, the wide Glastenbury terrace, which he here leaves out of consideration, proves it not to have been a fact.

tinct between High street and the next street west, north of the grounds of the Wesleyan University, and has a maximum height of about 150 feet; and these grounds, excluding a portion in the southwest corner, are part of it. A terrace two miles east and southeast of Middletown, above where the river narrows, I found to have the same height—150 feet.

Immediately around the city of Hartford the plain is mostly 40 to 60 feet above high flood-level in the river (70 to 90 above low-water); and in many places it consists of clay to the top. But beyond three or more miles there is a rise to higher levels.

Northwest of Hartford, along the Connecticut Western Railroad, the plain is extensive 6 to 7 miles out at a height near 130 feet, and between 9 and 9½ miles, at a height of about 160 feet.*

East of Hartford, the terrace has a maximum height near Manchester, 7 miles out, of 155 to 160 feet.

South-southwest of Hartford, the extended New Britain plain is 142 feet in height above the same base—high flood-level in Hartford.†

The facts appear to point to a height of about 160 feet for the upper terrace of the Connecticut valley, that is, for the glacial flood, in the vicinity of Hartford.

Sixteen miles south of Hartford, on the railroad route to New Haven, the broad divide between these two cities—1½ to 2 miles north of Meriden—has its highest part made by a terrace of stratified drift, (rising 20 to 21 feet above the railroad track); its surface is about 185 feet above mean high tide at New Haven, and 158 feet above flood-level at Hartford, and it is probably, therefore, a Connecticut River terrace;‡ and since it has a depth of more than 20 feet, the Connecticut during the glacial flood must have lost some of its waters by their spilling over the divide into the Quinnipiac valley and river.§

* I learn from Mr. W. H. Yeomans, the Superintendent of the Connecticut Western Railroad, that the height of the track 9½ miles out, near Scotland, is 190 feet above mean tide level, which is equivalent to about 160 feet above highest flood at Hartford.

† Mr. S. C. Pierson, of Meriden, has informed me that the height of the New Britain plain is about 172·25 feet above mean sea-level, which is 169·12 feet above mean high tide, and about 142 feet above highest flood-level at Hartford.

‡ A leveling from Meriden to the divide, for the height of the divide above the crossing of Main street and the railroad in Meriden, gave Mr. S. C. Pierson 43·64 feet; which, with 121·50 for the height above mean high tide, and 20 feet for the terrace, gives for the height of the divide above high tide, about 185 feet; or 158 feet above the Hartford flood-level. A careful barometric measurement at Meriden by Mr. E. S. Dana, obtained for the height of the track by the Meriden depot above high tide, 127·3 feet; which would give 191 feet for the whole height of the divide above high tide, and 164 feet for the height above Hartford flood-level.

§ The evidence favoring the view that waters from Farmington River poured down the Quinnipiac during the glacial flood is even more conclusive. Prof. Hitchcock has suggested (Rep. Geol. Mass., 329) that the Connecticut may have contributed to the Farmington along a route west of Mt. Tom and over Westfield and Granby.

At Springfield, within a mile or less from the river, there commences the great terrace of the region—a plain several miles wide—having a height, as I am informed by Mr. G. A. Ellis, city engineer, in general between 140 and 145 feet above flood-level in the river (the higher floods raising the river about 22 feet), or about 200 feet above high tide. But this wide terrace shows, by the fact that the material of it is mostly fine sand and clay, that it was made in comparatively quiet waters before the flood was at its height, and that it is probably not the highest. Beyond three miles southeast, east, and northeast of Springfield there are hills of stratified sand and gravel, rising to a height of 180 feet above flood-level, which belong to the *upper* terrace, and mark more nearly the flood's height. I learn from Mr. Ellis, to whom I am indebted for a map of Springfield and its vicinity for ten miles around giving the amount of elevation for a large part of the surface, that this 180-foot level (or 170 to 190) occurs in hills made of hardpan near the Boston and Albany Railroad, 3 miles from the Springfield depot; and south of this, near the "Old Bay Road." A level of 180 feet occurs along the same railroad between 6 and 9 miles from the depot, or for 3 miles east of Jenksville. Traversing the region with Mr. Ellis, he has shown me that the 180-foot level extends from Jenksville northward, with little variation, toward the Granby line, over four miles, if not beyond, and westward five to six miles to the Connecticut river. At Willimansett, on this part of the river, Prof. Hitchcock measured the height of the terrace and found it 172 feet above flood-level (or 194 feet above low water); and at South Hadley, two miles north of Willimansett, he found nearly the same height.

The upper terrace is therefore not less than 180 feet above flood-level in the river (or 237 feet above high-tide level in the Sound,) and probably this is very near the true height.

Ten miles east of Springfield, at Collins Station on the railroad, there is a plain, as Mr. Ellis informs me, at a height of 210 feet. But this is partly confined by the Wilbraham Hills, and is properly the terrace of the Chicopee river, which is but three-fourths of a mile distant. From Mr. Ellis I learn further that in Westfield, 10 or 11 miles west of the Connecticut, the land rises into a plain of great extent, being two miles broad, "widening between Westfield and the point where Westfield Little River leaves the town of Russel," which has a general elevation of 205 above flood-level in the Connecticut, or, 262 feet above high tide, "corresponding very closely with the plain-region the same distance to the east of Springfield." But this Westfield plain is in the Westfield valley, and is properly a Westfield river terrace. Yet if these higher flood-grounds, extending to a distance of 10 miles on the east and west, were outside of the limits of the Connecticut flood, they

indicate that the waters were pouring into the central trunk in vast volumes from the regions either side, and also that the same flooded condition characterized all New England valleys.

The depth of the flood-waters and their great width along the Connecticut valley cannot therefore be questioned. Now what does this great height of the upper terrace indicate with regard to the depression of the land when the deposits were made, or the elevation since that time? What are the "necessary deductions?"

1. Two miles below Middletown, the Connecticut river passes between high and steep rocky slopes, and has a width of but 400 yards; and when the old flood was at its height, the width was even then not over 500 or 550 yards, not a tenth of that at Middletown. These "Straits" or Narrows extend for two-thirds of a mile; there is then some widening on the east, and a mile below, as much on the west, giving room for the terraces of Middle Haddam and Maromas. Thence the valley continues southward with more or less room for a terrace on the west side and little or none on the east, the valley, reckoning to the limits of the terrace, seldom much exceeding half a mile in width.

The Narrows below Middletown, like the Narrows on the Housatonic below Derby, had evidently much to do with determining the great height of the flood-waters above. The waters could not pass off by the contracted outlet as fast as supplied by the melting glacier, although this outlet was over a fourth of a mile wide; and hence the waters were piled up high over the back region. This effect is imitated on a small scale with the melting of the winter ice and snow causing the spring floods; the water at Middletown increases its height at the highest floods about 25 feet, which is equivalent to increasing its average pitch thence to the Sound by five-sixths of a foot per mile.*

There is evidence that a very large part of the 150 feet was actually due to the flood. This is found in the descending

* The height of the floods at Hartford is 4 to 5 feet above that at Middletown. I owe to General Ellis the following table of flood heights at the two places. The low water at Middletown from which they are measured is 1·3 feet below the zero at Hartford, and the latter is ·045 foot below mean sea-level.

Flood of		1801,	Hartford,	27 ft.	6 in.	Middletown,	23 ft.	8½ in.
"		1843,	"	27 "	2 "	"	22 "	11 "
"	April,	1852,	"	23 "	1½ "	"	19 "	5 "
"	Nov.,	1853,	"	20 "	6 "	"	17 "	2 "
"	May,	1854,	"	29 "	10 "	"	25 "	8½ "
"	Aug.,	1856,	"	23 "	4 "	"	18 "	1 "
"	March,	1859,	"	26 "	5 "	"	21 "	10 "
"	April,	1862,	"	28 "	8 "	"	23 "	9½ "
"	March,	1865,	"	24 "	9 "	"	20 "	00 "

He remarks further that the average difference of the heights of the nine freshets is 4 feet 3½ inches, and the slope of the surface would be 1·3 feet more than the difference of the heights.

height of the uppermost terrace of stratified drift along the river to the Sound. The heights, on the west side of the Connecticut, going *southward*, are as follows:*

2 m.	below the Narrows,	the Maromas plain	133½	ft.
5 m.	"	near Higganum	100–105	"
7½ m.	"	near Haddam station	82– 85	"
10½ m.	"	near Goodspeed's station	75	"
14 m.	"	near Chester station	60– 62	"
20 m.	"	1½ m. below Essex	42– 45	"
22½ m.	"	near ferry and railroad	30– 31	"
24½ m.	"	Saybrook Plain	20– 21	"
26 m.	"	Seashore House, on Sound	13½	"

The plains, the heights of which are here given, were well displayed, leaving no doubt that they corresponded to the true terrace; and they were the highest in each region. They show that, from the Narrows, the river waters plunged along with a pitch of several feet a mile.

It may seem almost incredible that waters so violent should have made terraces in many places along the sides of the narrow valley. But the material of these terraces—mainly sand and coarse gravel, and the coarser above as usual—was to a large extent washed in from either side by the waters descending the side valleys, ravines, and all sloping surfaces; for there was gravel and sand everywhere over the hills from the dissolving glacier, and water everywhere in profusion for the work of transportation. The waters merely stratified and leveled off at top the material thus contributed. Moreover, we can follow the rise of the flood in the succession of the deposits; in clays, in some places, near the bottom—one bed supplying a brick-yard just below the Narrows—made when the waters were yet low; the sands next deposited while the waters were rising; the fine and coarse gravel mostly above—at the top of the lower, as well as of the highest terraces—spread out when the flood was at its height. The Middletown region is mostly one of fine earth, and the sand and gravel of the terraces below the Narrows could not have been carried over it by the stream, without making large depositions,—except in ice-floes, and these certainly did little of the transportation in the case here referred to.

Finally, how far was the height of the terraces described due to an elevation of the land since the beds were deposited?

Judging from the seashore terrace, on Saybrook point, the elevation of the land did not exceed 15 feet. I have not succeeded in finding other evidence on this point.

* The height of the Maromas plain was determined by levelling, by Messrs. Pillsbury and Clarke, of the Wesleyan University. They obtained 133½ feet above flood-level. The extended terrace on the opposite (east) side of the river has about the same height. The other measurements were made by the author.

4. The River Thames.

The conditions of the stratified drift deposits of Norwich are similar in several respects to those about Birmingham. Two rivers from the northward, the Quinebaug and the Yantic, pass either side of the city and unite below it to make the Thames. They rise in Massachusetts, and have nearly the same drainage-area as the two that join at Birmingham. They descend from high land and most of the way in rapids. Immediately below Norwich the valley is so narrowed by hills over 150 feet high that the width of the river, were the water 100 feet above its present level, would be hardly half a mile. But below these Narrows the valley expands, though still closely bounded by high land. The points in which the two cases differ are these:

(1.) The Narrows have greater width, and the modern stream below has much greater depth as well as breadth: the channel being one of the tidal, navigable fiords of the coast, the tides at Norwich having a range of more than three feet. Consequently it offered the flood-waters a less contracted way to the Sound.

(2.) The distance from the Narrows to the Sound is greater, being 14½ miles.

(3.) Although this distance is so much greater, the highest spring floods cause a rise above high tide of but 7½ feet—much less than half that in the Housatonic at Birmingham.

Again, (4,) the terrace plain of upper Norwich—the site of Broadway and its many fine residences—has over its northern part (at the foot of the little triangular Green) a height above tide level of 117 feet;* and, since the spring floods raise the water but 7½ feet above high tide, about 110 feet above flood level, against 95 feet at Birmingham.

The height of the Norwich plain above flood level diminishes from 110 in its northern part to 101 feet in its southern, a distance of about a mile; and this is very nearly the level of the terrace to the eastward of the latter at the Old Cemetery. The Norwich plain is so extensive, and the deposits so clearly consist of bedded sands and gravel, with the flow-and-plunge structure in some places quite to the top, that there is no occasion to doubt the statement that the water once stood at its high level. And since the deposits—those beneath the Old Cemetery, for example,—are made up in places of the coarsest gravel, the waters were the hurrying waters of a great torrent.

That this was the actual condition is proved further by the heights and nature of the terraces below Norwich. For they

* I have these numbers from the Water Commissioner of the city of Norwich, Mr. Winship. I have been much aided in the study of this region by a map of the Thames, giving contour lines for 50, 100 and 200 feet, which was furnished me by the Superintendent of the Coast Survey.

declined southward, precisely as in the Housatonic and Connecticut River valleys. The heights are as follows:*

Highest part of Norwich Plain,			110 feet.
1 m.	South,	on Norwich Plain,	101 "
2½ m.	"	at Thamesville, below the Narrows,	88 "
5½ m.	"	near Mohegan R. R. Station,	75 "
8 m.	"	½ m. W. of Montville R. R. Station,	61 "
10½ m.	"	in Smith's cove, 250 yds. W. of R. R.,	50 "
13½ m.	"	at New London, northeastern part,	24–25 "
14 to 16 m.	"	between New London and the Lighthouse,	none distinct.

Below the Narrows the valley opens widely on the west about Thamesville, and consequently has there a broad terrace. The same is true also of the region and terrace of Montville where a lateral valley comes in. The material of the upper part of the formation on the river near the Montville railroad station is very coarse, some layers being made up largely of cobble stones. On the point just north of Smith's Cove, the terrace is only 25 to 27 feet high; but it shows that it is below the normal height by its stony character; and, further, just around the point, 250 yards inside of the Cove, there is a much more extended terrace at a height of 50 feet, made of finer material.

The terrace in the northeast part of the city of New London (near where the track of the Northern Railroad leaves the city) is also very stony, and must be below its normal height, and probably as much as ten feet.

In passing the Narrows to Thamesville the decline of the terrace plain is 13 feet; and below Thamesville to Smith's Cove, on an average nearly five feet per mile. This last pitch if continued to New London would make the normal height there 35 feet instead of 25 feet. The terrace is equally low on the east side of the Thames in Groton; but it is also very narrow there, little room existing at the base of the hills for a terrace.

South of New London, between it and the Sound, I saw no distinct terrace.

The decline southward in the height of the terrace plain, as in the case of that along the other rivers, does not suggest marine action as the means of deposition. It manifestly points to the pitch of flowing waters in the channel, and to such a flow as could exist only in a period of incredible floods—when an outlet averaging over half a mile in width was not wide enough to discharge the waters coming from the hills and valleys within a range of forty miles.

* For the height of the plain near the Montville Station, I am indebted to a levelling by Mr. C. P. Jennings, Surveyor of the city of New London.

Direct evidence as to the actual level of the coast about the mouth of the Thames during the era is wanting; both because of the absence of a seashore terrace, and because no sections were exposed along the Thames that afforded a chance for studying satisfactorily the character of the bedding. But if the New London Lighthouse Point is without terraces, I have seen several examples of them at the head of broad bays between New London and Stonington, and in no case have I found the height over 15 feet, and generally it is but 10 or 12 feet. Again, Fisher's Island—about seven miles in length—lies off this same coast, and within five miles of it; and at the head of West Harbor, there is a well defined shore terrace of 15 feet; and, at East Harbor, one of less certain nature nearly 25 feet. 25 feet is therefore the greatest height inferable from this kind of evidence, and 15 may meet the facts.

5. Narragansett Bay.

Providence is situated on Providence River within a mile of the head of Narragansett Bay. Its terrace, averaging 80 feet in height above high water, is extensive, and part of the city is built upon it. The river opens into the broad northern arm of the Bay, and affords 14 feet of water for shipping to the city. The Bay has passages one to three miles in width among its islands, and enters the Sound about 26 miles south of Providence. With so open a passage for the waters we might infer that certainly the Providence terrace must mark a sea level of the Champlain period. But in view of the facts detailed in the preceding pages it is evident that something of this height is attributable to the flood descending the river valleys.

I have not been able to study carefully all the shores of the great bay with reference to its terraces. Ten and a half miles south of Providence, east of East Greenwich, there is a very wide terrace-plain, which extends south toward Wickford. Near the railroad in East Greenwich, the height is 56 feet, showing a loss of 24 feet of elevation in the 10½ miles from Providence. At Wickford, 7 miles farther south, the height is much less, little exceeding 30 feet.

There is a terrace at Fall River, on the west shore, about 17 miles from the Sound, and between this place and Tiverton. The height, just below the depot at Fall River, is 35 feet above high water; but the beds are very stony toward the top, and hence it is that the terrace is 30 feet below the normal height; some of the stones are a foot in diameter. The terrace south of Fall River has no greater height. About Newport there appeared to be no well defined terrace.

But direct evidence bearing on the height of the region in the Champlain period is afforded by the coast region between

Point Judith (the west cape of Narragansett Bay) and Watch Hill (the cape south of Stonington). Along much of this shore, as stated on page 411, there are wide flat meadows between the hills and the sea, having a height of about 11 feet above high tide. The road passes between these flat meadows on the south, and the bowlder-covered hills on the north, and the contrast is very striking: the former sandy or gravelly and thinly grassy among the great bowlders, the latter having an even dense turf over a rich black soil. Above the level of the meadows no trace exists of sea-shore flats or beaches; the stony hill-sides rise gradually, with nothing about them to suggest a Champlain submergence.

Off this shore, one to two miles, there is a sandy barrier; rising into hills to the westward, and it may be questioned, therefore, whether the meadows may not owe a part of their height to the flood waters. But the black soil appears to be evidence that they long lay with the surface at the sea-level, perhaps as a salt marsh of the Champlain period. Large portions are thickly strewn with broken oyster shells, left by the Indians, and this may be one source of the fertility.

No marine relics have yet been found in Champlain deposits about any part of Narragansett Bay to mark the sea-level. Such fossils should be looked for with more care than has hitherto been used; but much looking will probably end in finding none. The great glacier must have filled the channels among the islands; and as the ice disappeared, the floods, having a strong pitch owing to the height at Providence, would have made a profound sweep through them. Absence of marine fossils is therefore what should reasonably be expected.

An 80-foot terrace at Providence, a 56-foot terrace at East Greenwich, and an 11-foot plain on the sea-coast with no trace of any other terrace-level on the coast hills, are the positive facts gathered from the vicinity of Narragansett Bay.

6. Pre-glacial Sand-hills.

To define more clearly what are true deposits of stratified drift of the era of the melting in Southern New England, I add a few remarks on certain sea-shore sand-hills, that are easily mistaken for drift formations. I refer to ridges and hills of stratified material along the shores between Watch Hill and Point Judith.

It has long been known that Martha's Vineyard consists largely of Tertiary sands interstratified with clays, unconsolidated — except in some places through limonitic depositions making a limonitic or iron conglomerate. Block Island, Long Island, and Fisher's Island, just off the New England coast, are also made up to a great extent of such unconsolidated

pre-glacial beds,—as I shall more particularly explain in another memoir.

The New England coast region, from Watch Hill eastward, is but a continuation of Fisher's Island, in its features, and also, as there is reason to believe, in its Tertiary deposits. Its hills or ridges are of various heights, up to 180 feet; and nothing besides unconsolidated, though bedded, sand, gravel and clay, occurs in their constitution. Clay-beds are not in sight. But near the Ocean House, one of the large Watch Hill hotels, a pond, near the sea-level, is called Clay pond, because of the clay beneath the water; and I am informed by a resident in the region, that masses of clay are sometimes thrown up by the sea on the beach, showing that it exists at the base of the sand hills.

Over the stratified material of the hills lies the unstratified drift, with multitudes of large bowlders .

These hills show that they do not consist of Champlain or later deposits by the following characteristics.

In the first place, the hills are, as stated above, covered throughout with bowlders, down to within 10 or 12 feet of the sea level, and this demonstrates that the hills were there before the deposition of the bowlders and the associated gravel and sand.

Further, the features as to the bowlders and the hills are just those of Fisher's Island, which lies in their line at a distance of only three miles to the west;—and not that distance, since there are intermediate islets and reefs connecting the two; and on Fisher's island the clay beds and sand beds which underlie the top-dressing of unstratified bowlder drift are in some places upturned and folded—proving thus their anterior origin.

We may hence set aside those sea-border ridges as not of the Champlain period, and regard them as prior in elevation even to the Glacial period. Only the low grassy plain at their foot along the shores is of the Champlain water-arranged drift formation.

Conclusions.

The observations described in the preceding pages relate only to five of the river valleys of Southern New England. Although I have made no systematic measurements of terrace-heights in other valleys, I have seen enough in many of them to assure myself that all have the same class of facts to afford. The conclusions which are here reached with regard to the river floods are therefore conclusions for all Southern New England, and beyond this, I believe, for all New England, and other regions covered by the great glacier.

1. The first question before us is—What was *the amount of depression* in Southern New England during the melting of the glacier.

The evidence that some elevation took place in Southern New England after the Champlain period, based on the existence of elevated flats along its sea-border, we may regard as conclusive. The height of the marine terraces where not modified by fluvial action varies between 5 feet and 25 feet, and rarely exceeds 15 feet. Taken alone, this affords no ground for believing that the elevation was less than 10 feet or greater than 25 feet. I think that 15 feet is the most reasonable inference, for the portion of the coast considered in the preceding pages.

The evidence derived from the structure of the bedding in the New Haven region—that is, the direction of the dip in the flow-and-plunge portion (p. 418) appears to be good; but since it is difficult to believe that the coast region of the Champlain period should have had no flats at the water level, to be placed, by an elevation of 40 feet, at a height of 40 feet, it is hard, in the absence of such terraces, to set aside all doubt with regard to that evidence. Hence, although at present unable otherwise to explain those facts, I am led to hold the conclusion in abeyance. This doubt is in opposition to my former statements. But those statements were based mainly on a study of the New Haven region, and the few facts from Connecticut previously on record. A decline in the height of the terrace-plain toward the Sound down to only 5 feet of elevation at the coast, along an estuary receiving three streams swollen by glacial floods, seemed to be accounted for satisfactorily by the sweeping action of the floods, and the erosion by waves during the progress of an emergence. But now that we know that such a seaward pitch in the terrace plains exists along all the valleys of Southern New England, and that the sea-coast terrace is universally but 5 to 20 feet in height, those causes appear to be inadequate. It is difficult to believe that they could have prevented so completely the existence of shore plains above the low height mentioned, through the whole line of coast, even (1) where no rivers sent down their floods, and (2) where distant barriers protected the shores from the heavier seas. Were the land 40 or 50 feet below its present level, as hitherto supposed, some of the islands off the coast would still have been a shelter from the heavier waves; and many of existing shore-hills would have been islands, protecting an inner region wholly from violent seas; so that deposits at or near a 40-foot or 50-foot level might well have been formed. And yet none such exist.

In the present state of the facts I therefore think that the

argument for an elevation of only 10 to 20 feet is the strongest, although not yet decisive.

2. *The river-valley formations not marine.*—The ocean took no part in the formation of the river terraces. The pitch in the terrace plains of the lower Housatonic, lower Connecticut, and the Thames, is alone sufficient evidence against marine action in the matter.

3. *The height of the flood the chief cause of the height of the terraces.*—The facts from the valleys of the Housatonic, Connecticut and Thames prove that the river-valley formations—which now stand at so great a height above the river's surface—were not wholly, or for the greater part of their height, formed *when the land was at a much lower level than now;* but, on the contrary, that they were formed *when the river's waters were at a greatly higher level than now*, and as a consequence, chiefly of the glacial flood. If the whole elevation of the land since they were made is but 15 feet, then the rest of the elevation of the high terraces was owing to the height of the flood.

The facts hence teach that we may have high terraces along valleys, and many terraces, and yet none be due to an elevation of the land. Further, the height of the lower terraces of stratified drift is *comparatively* of little geological interest. For, in New England, they cannot be proved to have had anything to do with successive stages of elevations; and, in general, they were underwater flats of different levels; or they originated as levels up to which the flood, according to its rate of flow, built the stratified material, or down to which they swept off that which had before been laid down; or they mark the level of oscillations in the height of the flood after this had reached its height.

4. *Height of the streams during the flood.*—The height of these valley formations above *flood-level* has been stated on preceding pages.

The following table contains these heights, and also the corresponding heights above mean *high-tide level.*

	Above flood level.	Above high-tide level.
On the Housatonic, at Birmingham,	95 feet.	110 feet.
On the Connecticut, at Middletown,	150 feet.	170 feet.
" " at Hartford,	160 feet.	185 feet.
" " at Springfield,	180 feet.	237 feet.
On the Thames, at Norwich,	110 feet.	117 feet.
Head of Narragansett Bay, at Providence,	80 feet.	80 feet.

To obtain the actual height above high-tide level, these numbers should be reduced by whatever was the amount of depression of the coast region during the Champlain period—that is by 15 feet, if that was the true amount.

Another reduction also is required.

The heights of shell-bearing beaches on the coasts of Massachusetts, Maine, the Labrador coast, Lake Champlain, and the St. Lawrence, show that the amount of depression in the Champlain period (or of elevation since) *increased to the northward.* If this was true along the eastern coast of New England, it was probably true also for the interior; indeed, Lake Champlain and the St. Lawrence at Montreal are part of the interior. Since the height of these beaches at Montreal have been shown by Dawson to indicate a depression there of 500 feet, and since that on the Sound was certainly not over 45 feet, and probably not over 15 feet, the average rate of increase from the Sound northward was about *one foot and a half* a mile.*

Adopting this as the average rate, the amount of depression in consequence of it was increased at Birmingham, 10 miles from the Sound, by 15 feet.

At Middletown,	20 miles	in an air-line,	by 30 feet.
At Hartford,	35 miles,	"	by 52½ feet.
At Springfield,	58 miles,	"	by 87 feet.
At Norwich,	15 miles,	"	by 22½ feet.
At Providence,	28 miles,	"	by 42 feet.

Providence, though 26 miles from the Sound, is 28 miles from a line having the course of the shore meadows west of Point Judith.

With an increase northward, in the depression, of only 1 foot a mile, the depression derived from the source here considered, would have been *one-third* less at each of the places.

5. *Pitch of the stream during the flood.*—Owing to this depression, the pitch of the streams seaward was diminished. From the above numbers we obtained for the *average* pitch of the rivers to the Sound approximately:

8	feet	per	mile	below Birmingham.
4·5	"	"	"	Middletown.
5	"	"	"	Norwich.
1	foot	"	"	Providence.

In the highest modern floods, the pitch is about two feet below Birmingham; nine inches below Middletown; six inches below Norwich; and not over one inch below Providence.

* This subsidence, so much greater to the north over New England than to the south, must have occasioned, as I have stated in other places, nearly or quite a cessation in the movement of the glacier; and in this condition the ice appears to have melted away. One important consequence of this is the absence from New England of all terminal moraines. Terminal moraines are made from long-continued successive depositions of moraine-material (rocks, gravel, sand) at the terminus or melting-place of the *moving* glacier; and were there no motion none would form. On this account, the subject of terminal moraines has not been included in this memoir on "Southern New England during the melting of the great glacier."

Or, supposing the rate of increase of depression northward over *Southern* New England only one foot per mile, or less than farther north, the pitch would have been approximately:

8·5	feet	per	mile	below Birmingham.
5	"	"	"	Middletown.
5·5	"	"	"	Norwich.
1·5	"	"	"	Providence.

If the land were 15 feet below its present level along the southern coast of New England, and the average rate of increase of depression northward were 1 foot per mile, the height of the flood waters above high tide would have been

At Birmingham,	85 feet.
At Middletown,	135 "
At Hartford,	135 "
At Springfield,	164 "
At Norwich,	87 "
At Providence,	37 "

It hence appears that, with 1 foot a mile as the northward increase of depression of the land, the waters would have been at Hartford on the same level as at Middletown; and at Springfield, but 29 feet above the level at Hartford, instead of 38½ feet, the present difference; and the pitch from Springfield to Hartford would have been about a foot a mile. With 1½ feet a mile of increase of depression northward, the flood at Springfield would have been only 10 feet higher than at Middletown.

7. *Glacial conditions.*—Whatever the rate of increase northward in the depression, and whatever the amount of actual depression along the Sound, only a small part of the marvellous in the glacial flood is removed. The rivers in the lower parts of the Housatonic, Connecticut and Thames valleys were cataracts on a scale beyond all modern knowledge. The waters from the melting glacier must have been poured down the streams in vast volume to have piled to so great heights before outlets so wide and so deep. And such facts are but examples of a condition that prevailed generally over the glacier-covered lands.

6. *The Champlain a Fluvial period.*—It is to be remembered that the glacier consisted of the precipitated waters of many thousands of winters, each winter, too, a year in length. Hence, when the melting reached its height, some centuries of precipitated moisture were let loose at once. The results from the action of the great rivers of the era are registered in the height and width of the valley formations. The Champlain was eminently the Fluvial period of the earth's history, while *pluvially*, or as it respects rain, it may not have exceeded the present time.

This last section of my Memoir, which purported at the outset to treat of the amount of depression of the land during the melting of the glacier, has turned largely into a discussion of the evidences and effects of the Glacial flood—the subject of the first section. This is a consequence of the fact that the extent of the upper valley-terraces afford stronger testimony to the flood than the material of the beds, enabling us even to deduce the depth and spread and pitch of the flowing waters through the New England valleys, large and small.

In another paper I propose to present other facts on this and connected topics derived from Long Island and the associated lands off the Southern New England coast.

ART. LIII.—*Ammonia a constant contaminant of Sulphuric Acid;* by F. H. STORER, Professor of Agricultural Chemistry in Harvard University.

IN reflecting upon some of the reactions by which ammonia is known to be formed, it occurred to me that this substance would very probably, or perhaps necessarily, be produced in the ordinary process of making sulphuric acid, and that it might remain as a contamination in the acid as used in the chemical arts. Acting upon the idea I have myself tested and have caused to be tested carefully and methodically a considerable number of samples of sulphuric acid obtained from different chemical works, and have found that every one of the specimens examined contained appreciable quantities of ammonia. I find, moreover, on looking the matter up, that the observation is not new, inasmuch as Schœnbein* has stated, so long ago as 1862, that he found traces of ammonia in all the samples of oil of vitriol which he had tested for that substance. My experiments would nevertheless seem to be worthy of publication, both because they confirm Schœnbein's statement and because they go to show that ammonia is far more generally distributed as an impurity of chemical substances than has been commonly supposed hitherto.

The following acids were examined quantitatively by distilling a small portion of each of them with milk of lime, free from ammonia, and applying Nessler's reagent to the distillate, in the manner described by Wanklyn, in his "Water-Analysis," London, 1874.

I. Oil of vitriol from a carboy bought at Bay-Side Alkali Works, South Boston.

II. Oil of vitriol from the Chemical Works at North Billerica, Mass.

* Wagner's Jahresbericht Chem. Technologie, viii, 266.

III. Oil of vitriol from Eaton's Chemical Works, South Wilmington, Mass.
IV. Oil of vitriol, "chemically pure," from Trommsdorff of Erfurt.
V. Pan acid from the Works at North Billerica.
VI. Pan acid from Eaton's Works.
VII. Chamber acid from Eaton's Works.
VIII. Chamber acid from the Works at North Billerica.
IX. Chamber acid from the Merrimack Print Works at Lowell.

Five cubic centimeters* of the acid	Gave grams of ammonia (NH_3).
No. I	0·000150
" II	0·000075
" III	0·000245
" IV	0·000095
" V	0·000070
" VI	0·000140
" VII	0·000050
" VIII	0·000158
" IX	0·000090

Care was taken to procure the above samples of acids (excepting Nos. I and IV) directly from the works where they were made. Each of the specimens was kept in a full and tightly closed glass stoppered bottle until tested, and the portion tested was taken from the middle of the bottle. With the exception of Trommsdorff's acid (No. 4) all the samples are known to have been made from sulphur, i. e., not from pyrites. Several other samples of oil of vitriol of unknown origin were found to contain ammonia when tested qualitatively with Nessler's reagent, and with the even more delicate reagent of Einbrodt.† So too when applied, for the sake of control, to the distillate from acid No. 4 of the foregoing list, Einbrodt's test gave a very strong reaction for ammonia.

There are several ways in which sulphuric acid may be contaminated with ammonia. Some insignificant traces of this substance are of course contained in the air which is used for making the acid, and a still larger amount is often contained in the water that plays so important a part in the process of manufacture. It is not impossible indeed that nitrogen compounds in the water may sometimes be the cause of appreciable traces of ammonia in the acid. It is easy to conceive moreover that considerable quantities of ammonia may be formed in the apparatus of the sulphuric acid maker through reduction of nitric acid or other oxide of nitrogen that is necessarily present,

* The weight of 5 c. c. of the oil of vitriol was rather more than 9 grams in each instance; that of the pan acid was about 8½ grams, and that of the chamber acid rather more than 7 grams.

† Mercuric chloride in alkaline solutions. See Liebig & Kopp's Jahresbericht, 1852, v, 723 and 1863, xvi, 167.

and I find, in fact, by direct experiment, that ammonia is formed when warm dilute nitric acid is made to act upon lead or upon sulphur.

Action of dilute Nitric Acid on Lead.—A quantity of soft, clean commercial lead that had just been remelted was placed in a small glass flask and 50 c. c. of dilute nitric acid (sp. gr. 1·15) were poured upon it. The flask was closed against the air with a gas delivery tube, and after the action of the acid had ceased the solution was boiled with the milk of lime and the distillate tested for ammonia. But the reaction with Nessler's liquor was so strong that no estimation of the amount of ammonia could be made. A quantity of the lead (30 grams) boiled by itself in the milk of lime gave a distillate in which no ammonia could be detected by the Nessler test. On the other hand, 50 c. c. of the dilute nitric acid were found to contain 0·000025 gram of ammonia.

In a second trial 50 grams of the commercial lead were warmed during three hours with 50 c. c. of the dilute nitric acid. The solution was distilled with the milk of lime and the ammonia in the distillate was estimated by titration with standard oxalic acid:—0·002483 gram of ammonia was found.

In a third trial 25 grams of pure lead (from Marquart of Bonn,) were warmed with 50 c. c. of the dilute nitric acid and 0·003279 gram of ammonia was found in the solution of nitrate of lead.

Action of dilute Nitric Acid on Copper.—15 grams of clean copper clippings were gently warmed with 50 c. c. of the dilute nitric acid until there was no more action. The solution was distilled with milk of lime, and the ammonia estimated by Nessler's test. 0·00004 gram of ammonia was found.

Action of dilute Nitric Acid on Sulphur.—20 grams of powdered brimstone were added to 50 c. c. of the dilute nitric acid and the mixture was maintained at or near the temperature of boiling for three hours. On testing the liquid an abundance of ammonia was found.

In another trial, 20 grams of the powdered brimstone were mixed with 100 c. c. of the dilute nitric acid. The mixture was allowed to stand in the cold for 48 hours, and then boiled gently during 8 hours. On testing the liquor by the Nessler process 0·00225 gram of ammonia was found in it.

A small amount of nitrogen oxides may perhaps be reduced to ammonia in the process of sulphuric acid making by other deoxidizing agents, such as the organic impurities of crude sulphur,* or sulphuretted hydrogen,† or even by sulphurous acid, though in a single experiment in which sulphurous acid,

* See Wagner's Jahresbericht Chem. Technologie, x, 149.

† Compare Johnston, in Gmelin's Handbook, ii, 396.

evolved from copper clippings, was passed into dilute nitric acid (sp. gr. 1·15) for a couple of hours no ammonia could be detected in the liquid. The experiment of Schœnbein* moreover is to be remembered, in which ammonia, as well as sulphurous and sulphuric acids, was detected in water above which sulphur had been burned in the air. It would seem to be plain, however, that the substances previously mentioned must usually be the most efficient agents for the production of the ammonia.

Doubtless a good deal of the ammonia thus formed by reduction is destroyed again by reacting upon oxides of nitrogen in the liquid during the process of concentrating the weak sulphuric acid. Pelouze proposed long ago† to free the acid from nitrous contaminations by heating it with sulphate of ammonia. The reaction, though efficient, is simply incomplete.

The presence of ammonia in sulphuric acid having been proved, the question presents itself whether, in spite of the tendency of ammonia to change to nitrous and nitric acid through oxidation, traces of it may not occur in a great variety of the chemicals in whose preparation sulphuric acid takes part. The following list comprises several substances, chosen somewhat at random, which I have caused to be tested for ammonia, some of them for the sake of proving this idea, though mostly for a different purpose. The substances under examination were boiled with milk of lime that had been proved in each instance to be free from any trace of ammonia by long continued boiling, and testing of the distillates obtained from it. The distillates obtained after the addition of the substance to the lime were received in graduated tubes and tested successively with Nessler's reagent until they ceased to show any coloration.

	100 grams (or c. c.) of the substance contained gram of NH_3.
Nitric acid, taken from a carboy of pure acid from Bayside Alkali Works, 10 c. c. gave 0·000035 gram of ammonia (NH_3).	0·00035
Chlorhydric acid, the ordinary concentrated commercial acid, 10 c. c. gave 0·000015 gram of ammonia,	0·00015
Ditto from a carboy of pure acid from Bayside Alkali Works, 10 c. c. gave very little ammonia,	still less than the preceding.
Acetic acid, chemically pure, from C. White & Co., Philadelphia, 10 c. c. gave no reaction for ammonia,	0·00000
Potash alum, pure from Marquart of Bonn, gave strong reaction for ammonia,	much.
Ditto, pure from Trommsdorff, gave strong smell of ammonia when heated with lime. There was so much ammonia that it could not be estimated by the Nessler test, when 10 grams of the alum were operated upon,	much.

* Journ. prakt. Chemie, lxxxvi, 145. † Gmelin's Handbook, ii, 183.

	100 grams (or c. c.) of the substance contained gram of NH_3.
Sulphate of alumina, pure from Marquart, 20 grams gave too much ammonia to be measured by the Nessler process (as used by us), and so did 5 grams,	much.
Sulphate of iron (ferrous sulphate), 5 grams purified, from Powers & Weightman of Philadelphia, gave 0·00062 gram of ammonia,	0·01240
Ditto, common copperas, the last of an apothecary's barrel, 5 grams gave 0·00507 gram of ammonia,	0·10140
Ditto, copperas of unknown origin taken from a bottle in store room of Bussey Laboratory, 100 grams gave only 0·00053 gram of ammonia,	0·00053
Ditto, a sample of pure sulphate of iron from Marquart, gave a strong reaction for ammonia when tested qualitatively, and a reaction was obtained also from a sample of Marquart's sulphate ("pure by alcohol,")	
Sulphate of lime, pure precipitated, from Marquart, 35 grams gave 0·0002 gram of ammonia,	0·00057
Other samples of sulphate of lime, tested in a slightly different way, gave the following results:—545 grams of ground gypsum, obtained originally at a seed store but kept in a store-room of the Bussey Institution for a year or more, were percolated with pure water (two liters) until no reaction for ammonia could be detected and the percolate was boiled without addition of lime, 0·00392 gram of ammonia was found in the distillate. On adding lime a new portion of ammonia equal to 0·00008 was given off,	0·000734
20 grams of ground gypsum bought at a seed store and tested immediately on reaching the laboratory, gave 0·00003 gram of ammonia on being boiled in pure water, and no more ammonia came off on adding milk of lime,	0·00015
150 grams of plaster of Paris, taken from a box in the Bussey store-room, gave 0·0035 gram of ammonia on being boiled with water alone, and 0·00002 gram more of ammonia came off on boiling with lime,	0·00234
20 grams of plaster of Paris taken from a keg found standing in a recently built house, gave 0·000035 gram of ammonia on being boiled with water alone,	0·000175+
Sulphate of potash, chemically pure, from Trommsdorff, 20 grams gave only a faint trace of ammonia,	trace.
Sulphate of soda, chemically pure crystals from Marquart, 20 grams gave no trace of ammonia. This result was verified by many qualitative trials,	0·00000
Bisulphate of soda from Marquart, gave strong reaction for ammonia when tested qualitatively,	much.
Sulphate of copper, pure from Marquart, 10 grams gave 0·000105 gram of ammonia,	0·00105

	100 grams (or c. c.) of the substance contained gram of NH_3
Nitrate of potash, pure crystals from Marquart, 20 grams gave 0·000015 gram of ammonia,	0·000075
Nitrate of soda, taken from a bag of the crude nitrate of commerce, 20 grams gave 0·00004 gram of ammonia,	0·00020
Common salt taken from middle of a large lump of native rock salt bought at a grocer's shop, 20 grams gave 0·000022 gram of ammonia. Compare Vogel's detection of chloride of ammonium in rock salt (Gmelin's Hand-book, ii, 416),	0·00011
Ditto, pure from Marquart, 20 grams gave 0·0000425 gram of ammonia,	0·000213
Ditto, a sample prepared by dissolving pure crystals of carbonate of soda in pure chlorhydric acid gave no reaction when tested directly with Nessler's reagent,	none.
Phosphate of soda, pure crystals from Marquart, 20 grams gave no reaction for ammonia,	0·00000
Acetate of soda, pure crystals, 20 grams gave no reaction for ammonia,	0·00000
Carbonate of soda, pure crystals from Marquart, gave no reaction for ammonia when tested directly with Nessler's reagent,	0·00000
Carbonate of potash, pure from Marquart, gave no reaction when tested directly,	0·00000
Hydrate of soda, pure from Marquart, 5 grams gave no reaction for ammonia,	0·00000
Hydrate of lime, obtained by slacking the excellent quicklime from Brandon, Vt., 20 grams of the whitest portion of the mass gave no reaction for ammonia, while 20 grams of the grayest portion gave 0·00001 gram of ammonia,	0·00005
Chlorate of potash from Marquart gave 0·0001 gram of ammonia,	0·00050
Crude sulphur, from a box in Bussey store-room, 10 grams gave 0·00015 gram of ammonia,	0·00150
Flowers of sulphur from Marquart, 10 grams gave 0·00075 gram of ammonia,	0·00750
Sulphide of sodium from Marquart, 5 grams gave 0·001 gram of ammonia,	0·02000
Sulphide of potassium from Marquart, 5 grams gave 0·0009 gram ammonia,	0·01800
Sulphide of iron from Marquart, 5 grams gave 0·00085 gram of ammonia,	0·01700
Ditto, from a quantity of the foregoing that had just been fused in a Hessian crucible, 20 grams taken from the middle of the solid cake, gave 0·00025 gram of ammonia,	0·00125

Portions of the distillates from the sulphides of sodium, potassium and iron, on being tested with Einbrodt's reagent, for the sake of control, gave strong reactions for ammonia.

In order to avoid any confusion that might arise from coloration of the Nessler reagent by sulphuretted hydrogen, the following modifications of the ordinary process were employed in testing sulphur and the sulphides. In the case of sulphide of potassium and sulphide of sodium, the weighed substance was dissolved in about half a litre of pure water, free from ammonia, the solution was distilled and a quarter litre of distillate was collected, experience having shown that all the ammonia in the sulphide came forward in this amount of liquid. The distillate was acidified with a few drops of sulphuric acid, boiled until sulphuretted hydrogen had ceased to come off and then redistilled with milk of lime, the ammonia in the new distillate being determined with Nessler's reagent in the usual way.

In the case of sulphur and sulphide of iron the finely powdered, weighed substance was distilled with milk of lime, the distillate was acidified and boiled to expel sulphuretted hydrogen and finally redistilled in the manner just described.

Oxalic acid, "tertium depuratum" from Marquart, 20 grams gave 0·0003 gram of ammonia, ... 0·00150

Tartaric acid, from Marquart, 10 grams gave 0·0001 gram of ammonia, ... 0·00100

Vogel (Wagner's Jahresbericht, ix, 526) found 0·012 per cent of ammonia in a sample of crude tartar, ... 0·01200

Boracic acid, pure from Marquart, 10 grams gave 0·000055 gram of ammonia, ... 0·00055;

that is to say, rather less than might have been inferred from the fact that ammonium compounds accompany boracic acid in the Tuscan lagoons (see Gmelin's Handbook, ii, pp. 97, 98; Bechi, Wagner's Jahresbericht, ix, 355; Vohl, ibid. xii, 205, and ibid. (N. S.) i, 210.)

Though the figures in the foregoing table may seem small or even insignificant to persons unaccustomed to use Nessler's test, they are really large in several instances and noteworthy in all. It should be understood, moreover, that in working with Nessler's process it is easy to exclude ammonia from the water and from the other reagents that are employed and to avoid the ammonia of the air. It is hardly necessary to urge that the utmost care has been exercised in these respects in all the foregoing tests. A large proportion of the substances tested were taken from the tightly closed, particularly well-ground, glass-stoppered bottles in which they had been imported and which had never been opened until the time of applying the test, but it is noteworthy that this precaution seemed to be devoid of significance.

Those substances, such as sulphate of soda for example, which contained no ammonia when taken from freshly opened bottles, likewise contained none when taken from bottles that had been frequently opened and which had stood in a store-room with other chemicals during three or four years. I find naturally enough, that filter paper and other porous materials that have been exposed to the air of a laboratory are highly charged with ammonia, in the same way that the reagent bottles upon our shelves become coated with ammonium compounds, but it would seem, nevertheless, that there is a limit to the penetrative power of the ammoniacal gases. It will be noticed for that matter that a tolerably large proportion of the substances examined contained no ammonia whatsoever, and in general I have not observed that chemicals taken from their bottles at the moment of reaching the laboratory are any more liable to be free from ammonia than those which have been long in store.

A good idea of the relative importance of the ammonia found in the chemicals above described may be got by contrasting the figures given in the table with the amounts of ammonia that occur in natural waters as given by Wanklyn and by many other authorities, or by comparing the above results obtained from chemicals with the following statement of results that were obtained from rain water in this laboratory at the same time and by the same operator.

	Contained milligrams of		100 c. c. of the water contained grams
	Free NH^3.	Albumenoid ammonia.	free NH^3.
One litre of rain water taken from south cistern of Bussey Inst.			
April 20, 1875,	·532	·08	0·0000532
April 20, 1875,	·532	·06	0·0000532
April 22, 1875,	·480	----	0·000048
May 4, 1875,	·500	·10	0·00005
June 7, 1875,	·480	----	0·000048
June 7, 1875,	·475	----	0·0000475
One litre of rain water caught in dish on roof of Bussey Inst.			
July 23, 1875,	·100	----	0·00001
July 29, 1875,	·300	----	0·00003
One litre of rain water from snow caught			
April 20, 1875,	·147	·06	0·0000147

It is worthy of mention that water obtained by melting ice —such as sold hereabouts, as in all American towns, for domestic use—is remarkably free from ammonia. Thus a litre of water obtained by melting a block of Muddy Pond ice was found to contain only ·013 milligram of ammonia. In other words, 100 c. c. of the melted ice contained no more than

0·0000013 gram of ammonia. A litre of water obtained by melting the clear portion of a block of Jamaica Pond ice, contained ·04 milligram of ammonia, and a litre of water obtained by melting the cloudy porous portion of the same block yielded precisely the same amount, viz: at the rate of 0·000004 gram of ammonia in 100 c. c. of the water. By distilling off from a glass vessel a comparatively small proportion of the melted ice it is easy to expel these traces of ammonia and to obtain water that is practically free from that substance, such as is needed for making the standards of comparison upon which the Nessler test depends. It appeared in fact that before such distillation the mere melted ice was as free from ammonia as the water of deep wells in this neighborhood, that had been slowly boiled down in a copper still to four-fifths of its original volume for the express purpose of expelling ammonia. Both the melted ice and the purified well waters had to be distilled anew in glass vessels in order to obtain water that was completely free from ammonia, but the proportion of impure distillate to be thrown aside was no larger in the one case than in the other.

I am indebted to my assistant, Mr. D. S. Lewis. for his skillful coöperation in this research, and to my friend Mr. F. P. Pearson, chemist of the Merrimack Print Works, for a number of samples of acids.

Bussey Institution, Jamaica Plain, Mass., September, 1875.

Art. LIV.—*Abstract of a Memoir on the Origin of the Alps;* by Prof. Edward Suess, of Vienna.*

According to the views of the early geologists, still widely accepted, the origin of mountains is to be ascribed to the elevation of a molten or semi-molten mass which has thrown up the rocks along its axis, and crowded the upper strata to the right and left, forming in this way a mountain-chain. This principle has been applied to the Alps by Studer, and, in accordance with this it has been customary to speak of a middle zone, embracing the isolated central masses, with parallel subordinate zones to the north and south. The folding and banded arrangement in the outer chains has been ascribed to a mighty pressure which has been exerted in a northerly or southerly direction by the central zone, as it was elevated from below. The cause of the elevation has been left entirely unexplained.

This view is, however, at variance with all the facts observed. It may be true that the granites of the Alps are in great measure of eruptive origin; but they are unquestionably much

* Die Entstehung der Alpen, 168 pp. 8vo. Wien, 1875.

older than the Molasse of Lucerne, so that they can have had no influence in the dynamic changes in which it has been involved; moreover, with the exception of one or two unimportant cases, no example can be shown in which eruptive rocks have been the cause of change of position in the older sedimentary strata. Another argument against this view is found in the irregular, shattered condition of the central masses as contrasted with the even trend in the folds of the outer-lying mountain chains. A glance, for instance, at the position of the crystalline rocks of the Finsteraarhorn, overlying the younger strata, shows that the folding-over must have originated, not in the eruption or expansion of isolated central masses, but in some general, horizontal movement of the mountain-system as a whole.

In the general consideration of this subject it is to be explained that the term Alpine System is intended to include all the mountain chains, with their various branches, from the Jura Mountains to the Appenines in the south, and the Carpathians in the east—in other words all those mountains which show a constant predominance of certain trends or lines of directions. The western and northern limits of this extended region are formed by the older elevations of the Iles d'Hières, the eastern edge of the Central-Plateau of France, the southern extremities of the Vosges Mountains and the Black Forest, with the southern border of Bohemia. Within this limit, the Alps are developed with wonderful regularity, stretching in great curves from the end of one of these older mountain points to the next; and against them the rocks have been pressed up and shoved on in parallel lines, as against immovable barriers. An example of this action is furnished by the island of gneiss and *rothliegendes* at Dôle, which forms the southeastern continuation of the Vosges Mountains, where the dependence of the folds and fractures in the Jura on the distribution of the older rocks can be most clearly seen. The whole Jura Mountains have been here pressed up into many parallel bands, while on the other side of the obstruction the Jurassic deposits cover a wide area without showing any trace of this tremendous horizontal movement. This same principle is true of the Alps to the east, but it is to be noticed that in the Juras the rocks in the northern border are continued immediately beyond the limits of the mountains, while in the eastern Alps the rocks, which on the northern side tower over the plain, have as a rule no distinct continuation on the other side of it.

Again, each branch of the Alpine system is everywhere *one-sided*, not symmetrically formed; while, at the same time, the moving power was alike throughout. This point is especially well exemplified by the Appenines, whose structure deserves a

detailed description. To the north of Genoa the long lines of the Molasse and Flysch rise gradually from the Piedmontese plain, and extend southward in great curves. In the neighborhood of Bologna the Flysch forms the dividing line between the inner-Appenine depressions of Tuscany and the outer-Appenine region of Adria, and it stretches on in an unbroken course through the peninsula to the Gulf of Tarentum. Within this limit the limestone mountains extend uninterrupted from Spezia southward, embracing the Abruzzen, the Gran Sasso and the elevations of the Basilicata. Still within the line, on the western coast of Italy, are found the isolated fragments of the older crystalline rocks. As traces of the action of the mighty forces which caused this great horizontal shove, we may point to the wide areas of depression of the Tyrrhenian and eastern part of the Ligurian seas, while between the ruins of the ancient rocks the fissures are to this day in part open, on which are situated a long series of volcanoes, and along which earthquake shocks are propagated. Thus the Appenines show two sides differing essentially from each other—one the side of shoving and folding, the other of fracture and volcanic phenomena; the former is convex and continuous, the latter is interrupted by areas of depression.

The western Alps repeat the same contrast of a folded outer side, and an inner side of fracture, though here the volcanic mountains are wanting. At no point on the southern side of these western Alps can an equivalent of the long anticlinal of the Molasse be found; in no case can a profile be given which shall show an older middle zone flanked by symmetrical side zones. The Juras, too, are a model of a true one-sided movement, caused by pressure against an immovable foreign mass of older rocks. The fracture line is in this case turned toward the Alps.

The eastern Alps alone show a great series of Mesozoic and Tertiary rocks on their southern side, which might be regarded as belonging to the hypothetical southern zone. If we attempt, however, to compare the long series of regular folds, which are so conspicuous in the northern zone, with the rocks on the other side, we find that nowhere in the latter is there the slightest correspondence. The careful consideration of the relations here exhibited shows that the strata do not conform in strike with those of the northern zone. On the contrary, we are justified in concluding that this broad mountain girdle separates toward the east into several one-sided chains.

The same one-sided structure belongs to the Carpathians and the other branches of the Alpine system to the east and south. This principle established, it becomes clear that we must abandon the idea of a symmetrical structure—a middle zone with

two equal and corresponding side zones, and grant that the whole mountain-chain, from the Appenines to the Carpathians, is the product of a common force, which has acted more or less in a horizontal direction.

In regard to the age of the Alps, or more properly the epoch in which they were elevated, a somewhat different view must be adopted than that which was accepted in former years. It is now unquestioned that strata belonging to the middle Tertiary have shared in the general movement. This shows that the movements which have ended in the elevation of this chain continued up to a comparatively recent time. It is equally true, however, that the same regions have in much earlier times repeatedly suffered similar movements, as is shown by the position of the younger sediments on the oldest rocks of the same chain. The many examples of this truth which might be quoted show that, up to a time which extends far into the Mesozoic age, the region of the Alps was often the theater of great catastrophes. The greater abundance of eruptive rocks in the southern Alps shows that in the earlier times the course of action was essentially the same as in later epochs.

The consideration of all the subjects touched upon in the preceding paragraphs lead to conclusions which to a very considerable extent agree with those arrived at by Prof. Dana in his discussion of mountain-making in general.

The force which acted to produce the results, which we see to-day must have been a *horizontal* one, as is abundantly proved by a survey of all the facts. The exertion of this horizontal force was essentially influenced by resistance from four different sources: 1, from the presence of foreign masses of older rocks; 2, from the folding mass itself; 3, from the occasional introduction of older volcanic rocks, as granite and porphyry, in the moving mass; 4, finally, it appears that single mountain masses, like the Adamello or the red-porphyry, near Botzen, have exerted an essential influence on the development of the surrounding mountain region.

The examination of the various mountain regions of Europe not included in the Alpine system, gives confirmation of the views thus far expressed in regard to the one-sided nature of mountains, and the horizontal shove which has been the cause of their elevation. This is true of the Bohemian region, taken as a whole; of the Riesengebirge, the Erzgebirge, and so on.

[For a detailed discussion of the subject, reference must be made to the complete memoir, of which this is an abstract.]

The direction of the fracture lines varies from northeast to northwest, and the motion was mostly to the northward, though some isolated exceptions, in the case of a southerly movement,

exist in central Europe. If we look at the subject more broadly, however, and pass out of Europe to America, and then further study in as great detail as is now possible the great mountain-chains of Asia, we arrive at this grand conclusion: throughout, mountain-masses and mountain-movements are *one-sided*, and the direction of the movement is in general *northwest, north, or northeast*, in North America and Europe, but southerly, or southeasterly, in central Asia. There is no regular geometrical arrangement in the mountain chains.

Looking at the facts which have been stated, making only the supposition that an unequal contraction of the surface of the planet has taken place, we see that the simplest form of mountain consists in a fracture, which runs at right angles to the direction of the contraction; the fractured part moves forward in the direction of the force from contraction, while volcanic phenomena may manifest themselves at the line of breakage. The Erzgebirge forms an example of such a mountain-mass.

The second and most common form begins with a main fold striking across the direction of contraction, and inclined toward that. The breakage takes place in the fold in the line of greatest tension. If the force continues, the part of this fold in advance will be pushed still farther on, piling up before it the sedimentary strata into broader, subordinate folds, while the part behind sinks down, and, between its fragments, the volcanic phenomena appear. Thus it is with the Appenines. In case of an obstruction in front the mass may be turned aside, and, in fact, many complications may thus arise.

Still a third form consists in the formation of a large number of parallel folds which cover a considerable area, but with a steep line, as a rule, on the inner side of the fracture, while the volcanic phenomena are wanting. Here belong the Jura Mountains. Cases may also occur in which the width of the main-fold is so great that there results, not a mountain-chain, but a general mass-elevation; an example of this may be found in the recent changes of level observed on the Scandinavian coast.

In regard to the depth at which the contraction producing the lateral pressure took place, it may be safest to say that while in many cases the depth must have been very great, in others the contrary is true. Thus the movement which occasioned the fracture of the Erzgebirge must have taken place at a great depth, as also those which elevated the oldest rocks of the Alps. On the other hand, the shoving forward of the northeastern Alps, and the deviation in their direction, belong to a higher horizon, and the elevation of the Molasse to one still higher, while examples may also be given of foldings which must have been very shallow.

No influence of a radial contraction has been observed; nor is there any effect in the direction of the waves of contraction which can be attributed to the rotation of the earth.

In conclusion, it may be remarked that mountain-making as a whole can be regarded as a stiffening of the earth's surface, which process has been determined by the distribution of certain older rigid masses. These may be made up of mountain lines pushed up together, and crossing each other, as in Bohemia, or they may consist of wide extended surfaces whose strata, even the oldest, have retained their horizontal position, as in the great Russian plain. These primitive masses conform to no geometrical law either in outline or distribution, though they have determined the form and course of the folds which contraction has produced in the more pliant portions of the earth's surface between them. E. S. DANA.

ART. LV.—*Studies on Magnetic Distribution*; by HENRY A. ROWLAND, of the Johns Hopkins University, Baltimore.

(Continued from page 335.)

Table I. is from a bar $17\frac{1}{2}$ inches long with a magnetizing helix $1\frac{1}{2}$ inch long at one end, the zero-point being at the other. Table II. is from a bar 9 feet long with a helix $4\frac{1}{2}$ inches long quite near one end, the zero-point being at 1 inch from the helix toward the long end. Table III. is from a bar 2 feet long with a helix $4\frac{1}{2}$ inches long near one end, so that its center was $19\frac{3}{4}$ inches from the end on which the experiments were made, the zero-point being at the end.

TABLE I. Bar ·18 inch diameter. 0 at end of bar.

L.	Q'_ε. Observed.	Q'_ε. Calculated.	Error of Q'_ε.	Q'. Observed.	Q'. Calculated.	Error of Q'.
0	2·7	----	----	0	0·	0
3	3·2	----	----	2·7	3·5	+ ·8
5	2·0	2·0	0	5·9	6·6	+ ·7
6	2·5	2·4	− ·1	7·9	8.6	+ ·7
7	3·2	2·8	− ·4	10·4	11·0	+ ·6
8	2·7	3·5	− ·2	13·6	13·8	+ ·2
9	4·3	4·3	0	17·3	17·3	0
10	5·3	5·2	− ·1	21·6	21·6	0
11	6·5	6·5	0	26·9	26·8	− ·1
12	7·7	8·0	+ ·3	33·4	33·3	− ·1
13	9·5	9·9	+ ·4	41·1	41·3	+ ·2
14				50·6	51·2	+ ·6

$Q' = 2{\cdot}60(\varepsilon^{\cdot 213L} - \varepsilon^{-\cdot 213L})$.

$Q'_\varepsilon = r2{\cdot}60(\varepsilon^{\cdot 213L} + \varepsilon^{-\cdot 213L}) = {\cdot}554(\varepsilon^{\cdot 213L} + \varepsilon^{-\cdot 213L})$.

In adapting the formula to apply to the case of Table I, we may assume that at the end of the bar $s = \infty$ and $C = 0$, which is equivalent to assuming that the number of lines of induction which pass out at the end of the rod are too small to be appreciated.

In Table II. observations were not made over the whole length of the rod, and the zero-point was not at the end of the bar. It is evident, however, that by giving a proper value to s we may suppose the bar to end at any point. As the rod is very long, expressions of the form

$$Q' - C'' = C'\varepsilon^{-rL} - C'' \text{ and } Q'_\varepsilon = rC'\varepsilon^{-rL}$$

will apply.

TABLE II. Bar ·39 inch diameter. 0 at 1 inch from helix.

L.	Q'_ε. Observed.	Q'_ε. Calculated.	Error of Q'_ε.	$Q'-C''$. Observed.	$Q'-C''$. Calculated.	Error of Q'.
0	----	----	----	----	902·5	
1	71·7	70·8	− ·9	825·2	825·9	+ ·7
2	65·2	65·3	+ ·1	753·5	755·1	+1·6
3	59·5	60·2	+ ·7	688·3	689·8	+1·5
4	53·5	55·5	+2·0	628·8	629·5	+ ·7
5	51·2	51·2	0	575·3	574·3	−1·0
6	46·7	47·2	+ ·5	524·1	523·1	−1·0
7	43·2	43·5	+ ·3	477·4	476·0	−1·4
8	40·0	40·1	+ ·1	434·2	432·5	−1·7
9	37·2	37·0	− ·2	394·2	392·5	−1·7
10	34·7	34·1	− ·6	357·0	355·6	−1·4
11	31·7	31·4	− ·3	322·3	321·5	− ·8
12	29·5	28·9	− ·6	290·6	290·1	− ·5
13	25·7	26·6	+ ·9	261·1	261·2	+ ·1
14	25·5	24·6	− ·9	235·4	235·5	− ·9
15	22·0	22·7	+ ·7	210·9	209·0	+ ·1
16	21·5	20·9	− ·6	187·9	187·3	− ·6
17	20·0	19·3	− ·7	166·4	166·4	0
18	19·1	17·8	−1·3	146·4	147·1	+ ·7
19	32·5	31·5	−1·0	127·3	129 4	+2·1
21	27·5	26·7	− ·8	94·8	97·8	+3·0
23	23·0	22·8	− ·2	67·3	71·1	+3·8
25	18·5	19·4	+ ·9	44·3	48·6	+4·3
27	14·5	16·5	+2·0	25·8	29·0	+3·2
29	11·3	14·0	+2·7	11·3	12·6	+1·3
31				0	− 1·2	−1·2

$Q' - C'' = 983\varepsilon^{-\cdot 08135L} - 80\cdot 5 = 983(10)^{-\cdot 0353L} - 80\cdot 5.$

$Q'_\varepsilon = r983\varepsilon^{-\cdot 08135L}\Delta L = 80(10)^{-\cdot 0353L}\Delta L.$

In Table II. the observations were near the end of the rod, and were repeated several times. Neglecting the end of the rod, we have $s = \infty$.

In these tables we see quite a good agreement between theory and observation; but on more careful examination we observe a certain law in the distribution of errors. Thus in Table I. the

TABLE III. Bar ·39 inch diameter. 0 at end of bar.

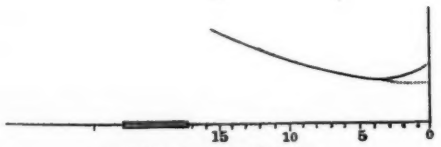
L.	Q'_ε. Observed.	Q'_ε. Calculated.	Error of Q'_ε.	Q'. Observed.	Q'. Calculated.	Error of Q'.
0				0·	0·	0
1	19·7	15·2	−4·5	19·7	15·2	−4·5
2	16·3	15·3	−1·0	36·0	30·5	−5·5
3	16·0	15·5	− ·5	52·0	46·0	−6·0
4	15·8	15·9	+ ·1	67·8	61·8	−6·0
5	16·5	16·3	− ·2	84·3	78·1	−6·3
6	17·0	16·9	− ·1	101·3	95·0	−6·3
7	17·6	17·6	0	118·9	112·6	−6·3
8	18·4	18·4	0	137·3	130·9	−6·4
9	19·2	19·4	+ ·2	156·5	150·3	−6·2
10	20·3	20·5	+ ·2	176·8	170·7	−6·1
11	21·8	21·7	− ·1	198·6	192·2	−6·4
12	22·8	23·1	+ ·3	221·4	215·3	−6·1
13	24·8	24·7	− ·1	246·2	239·9	−6·3
14	26·8	26·5	− ·3	273·0	266·4	−6·6
15	28·8	28·4	− ·5	301·8	294·6	−7.2
16	31·8	30·5	−1·3	333·6	325·1	−8·5

$Q'_\varepsilon = 7{\cdot}6(10^{{\cdot}037L} + 10^{-{\cdot}037L})$; $Q' = 89(10^{{\cdot}037L} - 10^{-{\cdot}037L})$.

errors of Q' are all positive between 0 and 8 inches; and this has always been found to be the case at this part of the bar in all my experiments.

The explanation of this is very simple. In obtaining the formulæ we assumed that the magnetic permeability of the bar μ was a constant quantity; but it has been shown by Dr. Stoletow and myself, independently of each other, that μ increases as the magnetism of the bar increases when the latter is not great. Hence between 0 and 8 inches the resistance of the bar R is greater than at succeeding points, and hence a less number of lines of induction pass down the bar from 8 towards 0 than would be given by the formula which has been adapted to the average value of R at from 9 to 14 inches. In Table II. this same fact shows itself towards the last of the table, and would probably be more prominent had the table been carried further. However, in this table all things have combined to satisfy the formula with great accuracy.

2.



Distribution at end of bar.

In Table III. we come across a fact of an entirely different nature from the above. Fig. 2 is the plot of this table, and gives the values of Q'_ε at different parts of the rod.

The horizontal line in the figure represents values of L, and the vertical ordinates are values of Q'_e. The full line gives the observed distribution, and the dotted line that according to the formula.

The formula gives the distribution very nearly for all points except those near the end. The formula indicates that Q'_e decreases continully toward the end; but by experiment we see that it increases near this point. On first seeing this, I thought that it was due to some residual magnetism in the bar; but after repeating the experiment several times with proper care, I soon found that this was always the case. I give the following explanation of it:—In the formulæ we have assumed R′, the resistance of the medium, to be a constant; now this resistance includes that of the lines of force as they pass from the rod through the medium and thus back to the other end of the rod; and of this whole quantity the part which affects the relative distribution at any part of the rod most is that of the medium immediately surrounding that part; and so the parts near the end have the advantage over those further back, inasmuch as the lines can pass forward as well as outward into the medium. The same thing takes place in the case of the distribution of electricity, where the "density" is inversely proportional to the resistance which the lines of inductive force experience from the medium; and here we find that the "density" is greatest on the projections of the body, showing that the resistance to the lines of induction is less in such situations, and by analogy showing that this must also be the case for lines of magnetic force. But this effect is not very great in cylinders until quite near the end; for Coulomb, in a long electrified cylinder, has found the density at one diameter back from the end only 1·25 times that at the center, and so there is probably a long distance in the center where the density is sensibly constant. Hence we may suppose that our second hypothesis that R′ is a constant will be approximately correct for all parts of a bar except the ends, though of course this will vary to some extent with the distribution of the lines in the medium; at least the change in R′ will be gradual except near the end, and so may be partially allowed for by giving a mean value to r.

Hence we see that could the formula be so changed as to include both the variation of R and of R′, it would probably agree with the three tables given.

To study the effect of variation in the permeability more carefully, we can proceed in another manner, and use the formulæ only to get the value of r at different parts of the rods.

No matter how r may vary, equations (2) and (3) will apply to a very small distance l along the rod; and as the origin of coordinates may be at any point on the rod, if Q′ and Q'_e are

taken at one point and Q and Q_ε at another point whose distance from the first is l, we shall have the four equations

$$Q = C, \qquad Q' = \frac{C}{A+1}(A\varepsilon^{rl}+\varepsilon^{-rl}),$$

$$Q_\varepsilon = \frac{A-1}{A+1}Crl, \qquad Q'_\varepsilon = rl\frac{C}{A+1}(A\varepsilon^{rl}+\varepsilon^{-rl}).$$

Calling $\frac{}{Q} = H$ and $\frac{Q'}{Q} = G$, we shall find, on eliminating C and A and developing ε^{rl} and ε^{-rl},

$$r^2 = \frac{2}{l^2}\left(\frac{GH+1}{G+H} - 1\right); \quad . \quad . \quad . \quad . \quad . \quad (9)$$

or to a greater degree of approximation,

$$r^2 = \frac{1}{l^2}\left(\sqrt{12\left(2\frac{GH+1}{G+H}+1\right)} - 6\right). \quad . \quad . \quad . \quad . \quad (9a)$$

Before applying these formulæ to any series of observations, the latter should be freed from most of the irregularities due to accidental causes. For this purpose the following Tables have been plotted and a *regular* curve drawn to represent as nearly as possible the observations; in other cases a column of differences was formed and plotted. In either case the ordinates of the curves were accepted as the true quantities. But for fear that some may accuse me of tampering with my observations, I have in all cases added these as they were obtained

The correction is necessary, because small irregularities in the observations will produce immense charges in r^2.

Table IV. contains some of the best observations I have obtained. It is from a bar 57 inches long with a helix $1\frac{1}{2}$ inch long in the center to magnetize it. Each quantity is the mean of six observations, these being made on both ends of the bar and with the current in opposite directions.

In this table a source of error was guarded against which I have not seen mentioned elsewhere. When a bar of iron is magnetized at any part and the distribution over the rest quickly measured, on being then allowed to stand some time and the distribution again taken, it will have changed somewhat, the magnetism having, as it were, creeped down the bar further. Hence in this table time was allowed for the bar to reach its permanent state.

On looking over column 6, which contains the values of $\frac{1}{r^2} = \frac{R'}{R} = R'\alpha\mu$ (equation 7), we observe that as Q′ increases, the value of $R'\alpha\mu$ first increases and then decreases. Now it is not probable that R′ undergoes any sudden change of this sort,

TABLE IV. Bar ·19 inch diameter. 0 at center of bar.

L.	Q'_c. Observed.	Q'_c. Corrected.	Q'. Corrected.	$r^2=\frac{R}{R'}$.	$\frac{1}{r^2}=\frac{R'}{R}$.
1			151·7		
2	24·0	24·0	127·7		
3	17·0	17·0	110·7	·041	24·4
4	13·7	13·7	97·0	·0256	39·1
5	11·6	11·65	85·4	·0192	52·1
6	10·2	10·15	75·2	·0168	59·5
7	9·0	9·0	66·2	·0150	66·7
8	8·0	8·0	58·2	·0153	70·4
9	7·1	7·15	51·1	·0150	66·7
10	6·4	6·35	44·7	·0142	62·9
11	5·7	5·65	39·1	·0160	62·5
12	4·9	5·0	34·1	·0167	59·9
13	4·4	4·4	29·7	·0180	55·6
14	3·6	3·9	25·8	·0184	54·3
15	3·3	3·4	22·4	·0184	54·3
$28\frac{1}{2}$	22·4	22·4			

and so it is probably due to change in the permeability of the rod. Hence by this method we arrive at the same results as by a more direct and exact method.* But by this means we are able to prove in the most unequivocal manner that *magnetic permeability is a function of the magnetization of the iron and not of the magnetizing force.* Hence it is for this reason that I have preferred in my papers on "Magnetic Permeability" to consider it in this way in the formulæ and also in the plots, while Dr. Stoletow in his paper (Phil. Mag., Jan., 1873) plots the magnetizing-function as a function of the magnetizing-force.

When we plot the results in this table with reference to Q' and $R'\alpha\mu$, the effect of the variation of R' is apparent; and we see, on comparing the curve with those given in my paper above referred to, that R' increases as L increases, at least between $L = 2$ and $L = 8$, which is as we should suppose, from the arrangement of the apparatus. For this table I happen to have data for determining Q in absolute measure, and these show that the maximum value of μ should be about where the table shows it to be.

This method of finding the variation of μ is analogous to that of finding conductivity for heat by raising the temperature of one end of a bar and noting the distribution of heat over the bar; and indeed the curves of distribution are nearly the same in the two cases.

If it were thought worth while, it would be very easy to obtain a curve of magnetic distribution for a rod and then enclose the whole rod in a helix and determine its curve of permeability.

* Phil. Mag., August, 1873.

This would give data for determining R′ in absolute measure at every point of the rod.

To complete the argument that the variation of r^2 is in great measure due to that of μ, I have caused the magnetizing-force on a bar to vary. Tables V, VI, and VII. are from a bar 9

TABLE V. Magnetizing current ·176.

L.	Q'_ε. Observed.	Q'_ε. Corrected.	Q′. Corrected.	$r^2=\frac{R}{R'}$.	$\frac{1}{r^2}=\frac{R'}{R}$.	Q''_ε. Calculated.
0	2·7					0
1	6·9					2·40
2	12·7	----	----	----	----	7·32
3	18·2	----	----	----	----	12·54
4	24·4	----	----	----	----	18·31
5	32·4	31·7	220·5	----	----	24·87
6	31·5	32·0	188·5	·0190	52·4	32·38
7	28·2	28·2	160·3	·0212	47·2	$Q''_\varepsilon=3.34(10^{\cdot 0236\varepsilon}-1-10^{\cdot 0236\varepsilon})$.
8	24·9	24·7	135·6	·0218	45·9	
9	21·4	21·7	113·9	·0236	42·4	
10	18·6	19·0	94·9	·0252	39·7	
11	16·8	16·4	78·5	·0298	36·0	
12	14·2	14·2	64·3	·0311	32·2	
13	12·0	12·0	52·3	·0367	27·2	
14		10·0	42·3	·0404	24·8	
15	17·7	8·2	34·1	·0440	22·7	
16		6·6	27·5	·0445	22·5	
17	11·6	5·1	22·4	·0570	17·5	
18		22·4				
End.	22·4					

TABLE VI. Magnetizing current ·31.

L.	Q'_ε. Observed.	Q'_ε. Corrected.	Q′. Corrected.	r^2.	$\frac{1}{r^2}$.	Q''_ε. Calculated.
0	16.3					0
2	22·0					17·3
3	32·4	----	----	----	----	22·3
4	43·8	----	----	----	----	32·28
5	55·9	----	----	----	----	43·34
6	55·2	----	391·9	----	----	55·90
7	46·8	55·1	336·8			
8		48·1	288·7	·0204	49·0	$Q''_\varepsilon=7.14(10^{\cdot 0236\varepsilon}-1-10^{\cdot 0236\varepsilon})$.
9	81·3	42·3	246·4	·0201	49·7	
10		37·4	209·0	·0202	49·5	
11	61·8	33·0	176·0	·0220	45·5	
12		29·0	147·0	·0243	41·2	
13	46·4	25·3	121·7	·0262	38·2	
14		21·9	99·8	·0300	33·3	
15	35·4	18·7	81·1	·0352	28·4	
16		15·6	65·5	·0405	24·7	
17	22·0	12·7	52·8	·0479	20·9	
18		9·8				
End.	43·0					

feet long and ·25 inch diameter. At the center a single layer of fine wire was wound for a distance of 1 foot, and the current for magnetizing the bar was sent through this. The zero-point was at the center of this helix and at the center of the bar, so that the observations on the first 6 inches include the part of the bar covered by the helix.

The values of Q'_ε are the sum of four observations on each end of the bar and with the current reversed. The three tables are comparable with each other, the same arbitrary unit being used for all.

TABLE VII. Magnetizing current 1·12.

L.	Q'_ε. Observed.	Q'_ε. Corrected.	Q'. Corrected.	r^2.	$\frac{1}{r^2}$.	Q''_ε. Calculated.
0	3.5		762·4			0
1	9·4	----	758·9	----	----	8·58
2	15·4	----	749·5	----	----	8·29
3	27·5	----	734·1	----	----	15·78
4	44·3	----	706·6	----	----	26·70
5	66·6	----	662·3	----	----	43·36
6	71·2	71·2	595·7	----	----	69·37
7	59·5	59·7	524·5	·0239	41·8	
8	51·0	51·2	464·8	·0200	50·0	
9	45·2	45·2	413·6	·0162	61·7	
10	40·3	40·3	368·4	·0141	70·9	
11	36·3	36·8	328·1	·0120	83·3	
12	33·3	33·5	291·3	·0107	93·5	
13	30·6	30·5	257·8	·0110	90·9	
14	28·1	28·0	227·3	·0116	86·2	
15	25·6	25·4	199·3	·0118	84·7	
16	23·4	22·7	173·9	·0140	71·4	
17	20·0	20·3	151·2	·0147	68·0	
18		18·1	130·2	·0161	62·1	
19	34·0	16·0	112·8	·0180	55·6	
20			96·8			
End.	96·8					

$Q''_\varepsilon = 35{\cdot}10 \frac{\varphi}{r^2} - 10 \frac{\varphi}{r^4}$.

Here we see an excellent confirmation of the results deduced from Table IV. In Table V, where the magnetizing-force is very small, and where, consequently, no part of the iron has yet reached its minimum resistance, the value of $\frac{1}{r^2} = \frac{R'}{R} = R'\alpha\mu$ decreases continually as the value of Q' decreases, as it should do. In Table VI, with a higher magnetizing-power, which was sufficient to bring a portion of the bar to about the minimum resistance, we see that $\frac{1}{r^2}$ remains nearly stationary for a short distance from the helix and then decreases in value. In Table VII, where the bar is highly magnetized and the portion near the zero-point approaches the maximum of magnetization, $\frac{1}{r^2}$ in-

creases in value as we pass down the bar, and having reached its maximum at $L=11\frac{3}{4}$ nearly, it decreases. These tables, then, show in the most striking manner the effect of the variation of the magnetic permeability of iron upon the distribution of magnetism.

It is evident that these tables also give the data for obtaining the relative values of R′ at different parts of the bar; but the results thus obtained are conflicting, and will need further experiment to obtain accurate results. Where such a small magnetizing force is used as in Table V. it is almost impossible to attain accuracy, and allowance should be made for this in deducing results from it. The greatest liability to error is of course where the magnetization is small; for any small residual magnetism which the bar may contain will be more apparent here, although great care was taken to remove all residual magnetism before use. Besides this there are many other disturbances from which the higher magnetizing-powers are free.

If we accept Green's formula as correct, *these observations give us data for determining the magnetizing-function of iron in a unique manner*, for nearly all other methods depend on absolute measurements of some kind. Thus the least value of r^2 in Table IV. for a rod ·19 inch diameter is ·0142, which gives $p=·01132$, which in Green's formula (equation 8) gives $\mu=33·8$ for the greatest permeability of this iron; and this is as nearly right as we can judge for this kind of iron. It is to be noted that Green's formula has been found for the portion of the bar covered by the helix, but as seen from my formulæ it will approximately apply to all portions, though it would be better to find a new formula for each case.

We shall toward the last resume this subject again, and so we will leave it for the present.

The results which I have now given, and indeed all the results of this paper, have been deduced not only from the observations which I publish, but from very many others; so that my Tables may be *considered to represent the average* of a very extended series of researches, though they are not really so.

[To be continued.]

ART. LVI.—*Notes on a New Feature in the "Comstock Lode;"* by G. P. BECKER. Ph.D., etc.

THERE is probably no metalliferous vein in the world which excites a greater interest than the "Comstock Lode" of Nevada, whether regarded from a scientific point of view, as the largest and richest argentiferous vein yet discovered, or from an

economical standpoint, as the source for many years past of an immense proportion of the silver, and a considerable fraction of the gold added to the circulation of the world; and the details of its geological character as developed, therefore acquire an importance not possessed by those of less prominent deposits.

The Virginia Range is a due north and south branch of the Sierra Nevada system of mountains. On the eastern slope of Mt. Davidson, the chief peak of the range, were found the outcroppings of the vein which have been followed somewhat to the north, and a long distance to the south of that mountain, and shown to possess a total length of four miles in the general direction of the Virginia Range. Decidedly the most important portion of the Lode—that in which the celebrated "Gould and Curry," "Hale and Norcross," "Ophir," "Consolidated Virginia" and "California" mines as well as others have been opened—lies at the base of Mt. Davidson, and is included within the limits of Virginia City.

It is well known to those who have had occasion to make themselves familiar with the Comstock lode* that throughout that portion of the vein which lies in Virginia City, the west or foot wall is a continuation of Mount Davidson, and consists, like the mass of that mountain, of syenite, while the east wall is a porphyry, more nearly described as trachytic greenstone, or propylite, with which the country was overflowed during the Tertiary period. The direction of the fissure subsequently filled by the vein matter of the Comstock, was plainly determined by the previously existing surface of contact between the syenite and propylite, which naturally offered less resistance to rupture than the solid mass of either adjoining rock. This is clear from the fact that for over a thousand feet from the surface, only insignificant masses of either rock were found on the side of the vein opposite to that of which they are especially characteristic. So strong was the influence of the direction, both in strike and in dip, given to the fissure by the presence of this comparatively weak surface of contact in the rocky mass, that the fissure extending in both directions away from Mount Davidson into solid propylite, unaccompanied by syenite, retained the strike, (nearly north and south,) and the dip (from 35° to 50°,) of the Virginia portion, almost unaltered.

This very clearly defined influence of the contact surface, was of itself good reason for supposing that the fissure in Virginia must follow the dividing plane between the syenite and propylite to a very considerable depth, and not improbably to one beyond the reach of mining operations. Nevertheless, although the subject has been very wisely left in abeyance by the geologists who

* Vide von Richthofen, "The Comstock Lode, San Francisco, 1865," and "U. S. Geological Exploration of the 40th Parallel," Vol. III.

have publicly discussed the Comstock, it cannot have escaped any of them that the fissure could not indefinitely follow this contact; for the propylite is recognized by them as a volcanic rock which has overflowed the older syenite, and therefore, unless we should make the highly improbable assumption that the syenite, (which in Virginia is plutonic,) the propylite, and the vein matter emanated successively from a single fissure in the earth's crust, a lowest point in the contact between the underlying syenite and the overlying propylite must eventually occur, from which the fissure would naturally descend toward the seat of the force by which it was caused. From the most depressed point in the contact surface, the fissure would pass into the underlying, or syenite rock, and doubtless out of it farther below, into whatever might underlie it in its turn.

From the point where the fissure leaves the contact between the two species of rock, its dip would in all probability be controlled by circumstances very different from those which decided it in the position lying near the croppings; for from the point at which the seismic shock impinged upon the lower surface of the syenite, the fracture must have followed the direction of least resistance, or presumably a straight line, and the shortest line from the point in which the subterranean force encountered the syenite, to the upper surface of that formation; and a moment's thought will serve to show that, apart from local and altogether incalculable irregularities in the lower surface of the rock, this line could continue in even approximately the direction of the present dip, only in case the syenite not only formed a cone above the general level of the country, but filled a vast depression below it as well. The passage of the lode into the syenite would therefore naturally be accompanied by a change in dip, toward the perpendicular.

In May last I made observations, repeated and extended in August, which seem to establish beyond question the hitherto unnoticed fact that the lower workings of the mines in Virginia City, have reached the point at which the fissure passes from the contact surface between the syenite of Mt. Davidson and the country propylite, into the underlying syenite. The depth below the surface at which the change occurs is not very far from 1500 feet.

The details of these observations would scarcely be of general interest away from the Pacific Coast, while the conclusion drawn will probably be considered as amply justified by the following general results of examination, viz: The east wall of the lode in the lower levels (1400–2300 feet) wherever struck between the north end of the "Ophir" and the south end of the "Chollar Potosi," a distance of over a mile, is syenite; that in the three mines, of the eight within these limits, in which for fear of water the East

Clay has not been pierced, good grounds (presence of included syenite and absence of included propylite) exist for believing that the hanging wall is syenite:—that although the east country syenite has been pierced in a number of places, both vertically and horizontally, to a depth of several hundred feet, no other formation has been reached, nor any indication that the limits of this have been approached; on the contrary, this eastern syenite is apparently as solid as Mount Davidson itself:—that wherever the relations between the walls of the fissure have been most completely exposed, the occurrence of syenite on the east wall is accompanied by a very decided increase in the angle of dip.

The last point is most clearly shown in the Ophir, where on the 1700 feet level the walls are almost exactly perpendicular, as shown by the very complete prospecting on that level.

The Gold Hill mines seem nowhere to have reached the syenite, as was to be expected, since from the conformation of the country, the syenite, if it underlies the porphyry in Gold Hill as well as in Virginia, will probably be met with only at a much greater depth.

To establish the exact line of the passage of the vein from the propylite into the syenite, would of course require a specially authorized examination of the mines; since a majority of points in which the east wall has been struck are hidden from view, either by the closure of drifts no longer essential to the working of the mines, or by timbering in the shafts, etc.

Many important deductions might be based upon a change in the conditions of the Comstock lode, which must be fraught with exceedingly important consequences to the greatest mining interest of the Pacific coast; but I prefer to confine myself to submitting the fact of this remarkable, and at this time most unexpected, alteration in the character of the vein.

Berkley, California, October, 1875.

ART. LVII.—*Notice of New and Interesting Coal Plants;* by E. B. ANDREWS.

[Read at the Detroit Meeting of the American Association.]

TWO or three years ago, I noticed in a ditch by the roadside in the western part of Perry county, Ohio, a thin layer of bituminous shale. Its stratigraphical position is near the base of the Ohio Coal-measures, perhaps thirty feet above the Maxville limestone, the Ohio equivalent and representative of the Chester limestone of Illinois. A few strokes of the hammer revealed a fragment of a coal plant entirely new to me. This led to subsequent visits to the locality and the discovery of a large

number of new forms of ancient vegetation. At the bottom of the layer is found less than an inch of a very peculiar substance of vegetable origin of brown color, soft like rotten wood, without lamination and filled with fragments of a minute form of plant resembling an *Asterophyllites*. In it are fish scales indicating, according to Dr. Newberry, a new genus and species, and also a small *Lingula*, perhaps too indistinct for specific determination. Above the half-solidified brown band we find an inch of highly bituminous laminated cannel shale, presenting a satin surface in its fracture. This shale contains a few plants, the most numerous form being leaves of *Lepidodendron*. This shale passes upward into an ordinary bituminous shale, in the lower two inches of which nearly all the new plants are found.

We have here the evidence of a marsh in which there was acccumulated upon a micaceous sandy bottom the minute moss-like calamitoid plants. These were buried by a highly bituminous sediment into which fell innumerable leaves of *Lepidodendra*, long straight leaves, often a foot in length. Then came in a luxuriant vegetation, chiefly ferns, which are buried in the higher shale.

So far as I know, this marsh must have been of very limited extent, for I have found no trace of it in the same geological horizon elsewhere in the neighborhood. The shale, on its outcrop, is so deeply buried by soil that so far only a few square yards have been uncovered and examined.

In this little marsh grew plants of well-marked Devonian types, and others of a type generally found in formations more recent than the Coal-measures. Besides these, there are many Coal-measure forms, but with scarcely an exception, they are of new species.

Of the Devonian types, one is a new species of *Archæopteris* Dawson, (*Palæopteris* Schimper.) The *A. Jacksoni* Dawson, from the Devonian of New Brunswick and Maine may be regarded as the typical form of this genus. The new Ohio species I have called *A. stricta*. It is a fern of great beauty, smaller and more delicate than *A. Jacksoni*. The pinnæ are alternate, somewhat closely set, growing out of the rachis at an angle of 70° to 80°, rarely as small as 45°. The pinnules are alternate, oblanceolate, obtuse, decurring on the narrow rachis, disconnected to the base, with a strong nerve, dividing near the base into three to five branches, which themselves fork once or twice before reaching the margin. Prof. Fontaine reports the finding of *A. Jacksoni* (or possibly a closely-allied species), in his conglomerate coal series on New River, W. Va., over a coal seam perhaps 500 feet above the base of the Coal-measures. This is more than 1500 feet lower in the series of the Alleghany

Coal-measures than my Ohio form. He also finds the same *A. Jacksoni* in Vespertine slate in Greenbrier Co., W. Va.

Another Devonian genus found in the little marsh in Perry county is the *Megalopteris* Dawson. The only species of this genus known was first found in New Brunswick by Prof. Hartt and described by him as *Neuropteris Dawsoni*. Prof. Fontaine has recently found the same in the lower Coal-measures on New River, where he found the *A. Jacksoni*. Of this genus I have found four new species, which I have called *Megalopteris Harttii*, *M. ovata*, *M. lata* and *M. minima*. The first three of these are closely allied to *M. Dawsoni* Hartt. The last, *M. minima*, has much the appearance of an *Alethopteris*, but it is undoubtedly a true *Megalopteris*. Some of the new species were ferns of large size and of great beauty. This genus has special interest and value because found in the Coal-measures of Ohio, only about 700 feet below the great Pittsburgh seam of coal. The West Virginia species found by Prof. Fontaine came from a horizon more than 1500 feet lower. But from this horizon, which is low in the original geosynclinal trough or depression in West Virginia (described by me in another paper read at Detroit and published in this Journal for Oct.), it is a long way down to the Devonian formation, which, in New Brunswick, yielded to the hammer of Prof. Hartt his *Megalopteris Dawsoni*. The new species from Ohio are directly separated from any Ohio Devonian rocks by, first, a few feet of Coal-measure strata, second, by the lower Carboniferous Maxville limestone, the equivalent and representative of the Chester limestone of Illinois, there 600 to 800 feet thick, and third, by the Waverly sandstone, over 600 feet thick. These facts are of no little interest as showing the distribution in the great time-series of this genus of ferns. If, however, we should apply the republican rule of majorities to science, the finding of several species in the Coal-measures and but one in the Devonian would determine this to be a true Coal-measure form. Then the Devonian species becomes the prophetic one.

Growing in our little marsh with the *Archæopteris* and the *Megalopteris* is a new and beautiful fern of a new genus of a more recent type—the *Tæniopteridæ*. No form of this order has hitherto been found so low in the Coal-measures. *Tæniopteris multinervis* Weiss, belongs to the upper Coal-measures and Permian of Europe, but most ferns of this order are of Mesozoic age. The only American representatives of this group, so far as I know, are *T. magnifolia* Rogers, from the Triassic coal-field near Richmond, Va., used with other plants by Prof. W. B. Rogers to determine the geological age of that field, and *T. vittata* Brongt., reported by Pres. Hitchcock, from the Red Sandstone of the Connecticut valley. The new Ohio genus I

have called *Orthogoniopteris*, from its rectangular nervation. The following is a description of the genus:

Frond simply pinnate. Pinnæ alternate, lanceolate or oblong, linear, rounded or tapering to an acute point, entire or undulate, enlarged and decurrent on lower side, rounded on the upper to the median nerve, and joining it a little above the point of its attachment to the rachis. Median nerve prominent, thick and ascending to the apex. Nervules forking once very near the median nerve, extending at right angles to it, and curving upward slightly at the margin, very fine, numerous and uniform.

Two species have been found, *O. clara* and *O. Gilberti*. This genus is allied to *Tæniopteris* Brongt., to *Angiopteridium* Sch., and to *Neriopteris* Newb. The nervation is similar to *Tæniopteris*, but *Tæniopteris* has a simple frond, while this is pinnate. In *Angiopteridium* the frond is pinnate and the pinnæ are cordate or rounded, with a marginal fructification. In Dr. Newberry's new genus, *Neriopteris*, the pinnæ are similarly cordate with acute-angled nervation and with a supposed marginal fructification. In this new genus the pinnæ are decurrent below, and free and rounded above, with a perfectly rectangular nervation. In the decurrent base of the leaflets it is allied to the larger forms of *Alethopteris*, but it doubtless belongs to the order of *Tæniopterideæ*.

Besides those already referred to, the bed affords a new species of *Alethopteris* greatly resembling *A. tæniopteroides* Bunbury, from the coal field of Cape Breton, but specifically different. Another fragment of a supposed *Alethopteris* also possesses a nervation much like the *Tæniopterideæ*. If a true *Alethopteris*, it is of larger leaf than any other species of this genus except *A. ingens* Dawson, from which it differs somewhat in nervation. There is another *Alethopteris*, which I have called *A. Holdeni*, of great size and beauty, belonging to the section of the genus which may be represented by *A. Serlii*, and is doubtless allied to Dr. Newberry's new species, *A. macrophylla*. The essential characters which distinguish it are: first, the great length of the frond, which measures at least fifty centimeters; second, its lanceolate, or rather oblanceolate form, the leaflets decreasing in length toward the base; third, the linear taper-pointed form of the leaflets, comparatively long and narrow; and fourth, the always simple division of the frond. It is possible that we have in these various species of *Alethopteris*, and in closely allied forms, a group which should be detached and formed into a new genus. This group passes, in resemblance, into the type of the *Tæniopterideæ* on the one hand and through the *Megalopteris* (the *M. minima* having much the look of an *Alethopteris*) unto the distinct *Neuropteris* type on the other. These resemblances are of great interest

and seem to bring the Devonian and Mesozoic flora into greater apparent harmony.

Besides the forms already mentioned the bed affords two new species of *Asterophyllites*, a new and beautiful *Hymenophyllites*, a new and pretty *Eremopteris*, two species of *Lepidodendron*—one allied to *L. tetragonum* Steenb., found by Dr. Dawson in the lower Carboniferous of Canada—two species of *Cordaites*, one probably *C. Robbii* Dawson, a Devonian form from New Brunswick, a new and large *Cardiocarpon*, a large fine cone of somewhat doubtful affinities, a curious jointed root and several other forms not yet determined. The most important of these plants are figured and will appear in the next volume of the Ohio Geological Reports.*

ART. LVIII.—*Letter to the Editors*, from Dr. B. A. GOULD, Director of the Córdoba Observatory, dated Córdoba, Sept. 8, 1875.

MESSRS. DANA & SILLIMAN:—When I took leave of you last November I did not intend that so long an interval should elapse without the fulfillment of my promise to send you tidings of the progress of my work. But what with the accumulation of material during my absence, and the relentless pressure of the current observations, I have been unable to do more than keep pace with the stream until now.

But now I have the gratification of informing you that the zone-observations are completed. There are 754 zones, containing somewhat more than 105,000 observations; the first zone having been observed 1872, Sept. 9, and the last 1875, Aug. 9, but as there was an interruption of about a year during this interval, not quite two years of work is comprised in the series. The measurement of the chronograph sheets and the transcription of the time-records is now completed; and the copy of the observations upon sheets ready for the printer is so far advanced that it will probably be completed before this letter can reach you. The second copy, upon computation-blanks, is likewise well under way.

Thus the entire region from 23° to 80° of South declination has been well scrutinized. The ten degrees around the pole have been so thoroughly examined by Gillis in Santiago and Stone at the Cape of Good Hope, that no zone observations seem needful there; and my northern limit overlaps Argelander's southern zones by eight degrees, in compliance with the special request of that deeply lamented astronomer.

The width of the zones has varied with their declination, and with the abundance of their stars; the maximum width being 2° at the northern limit, and 4° for the zones immediately north of

* Professor Andrews has sent us copies of his plates, and we can testify to their beauty and interest.—EDS.

75°, between which declination and 80° only one series was taken. These widths were always halved within the limits of the Milky Way, and still farther subdivided into quarters, and even eighths, where the richness of the region required; so that some zones have only comprised 20′ or 25′ of declination exclusive of their marginal overlap. The maximum number of stars contained in a single zone is 293, the average is about 140. It is needless to say that there are many repetitions, arising not only from the overlap, (usually 10′ at each margin), and five or six minutes of time at the beginning and end), but also from the re-observation of such zones as had proved unsatisfactory, whether from unfavorableness of weather, insufficient determining stars, or inadequate performance of circle, chronograph or clock.

It has been my unfailing rule to make the determinations as nearly absolute as circumstances would allow; and with this view, each zone has been preceded and followed by a series of observations for instrumental corrections,—consisting of transits of two standard time-stars, as well as of one circumpolar star above and one below the pole, together with measurements of nadir, collimation and level. But although these precautions have been taken, I do not purpose dispensing with the advantage to be derived from direct comparison with independent and sharply determined star-positions, and the catalogue of these is advancing very satisfactorily. My original plan implied the determination of some half dozen stars in each zone, selecting so far as it might be convenient, such as occur in the Uranometry. These would form a catalogue of about 3,000 stars, each of which was to be observed four or five times with all possible care. But when the zone-work was interrupted by my departure for home in 1874, I extended the list considerably, and thanks to the faithful and efficient labors of my assistants, this work was vigorously prosecuted during the year; so that there now remain comparatively few stars of the Uranometry, whose positions have not been independently determined in Córdoba. I purpose to employ the Meridian Circle for extending and improving this catalogue, while the reduction of the zone-observations, and the publication of the Uranometry are going forward, and have now extended the working list yet more, so that this catalogue will contain more than ten thousand stars, if all goes well, each star being observed on the average about four times. Up to the present time the total amount of different stars observed for this catalogue exceeds 7,000, although not all of them have as yet the requisite number of observations.

Besides the data thus collected I have a considerable amount of material resulting from the repeated observation of clusters. These observations, when properly reduced, will essentially aid the study of the photographic plates.

Now I must apply myself to the more laborious and difficult part of the work—the computation and preparation of the crude material obtained. This would be more difficult everywhere as requiring a more unremitting diligence, and careful organization,

but is doubly so in this country, where to the task of discovering persons fit to serve as computers, is added the labor of training them; and the dull routine of numerical computation naturally fails to enlist that popular appreciation and interest, which the process of instrumental exploration readily awakens, and in which the sinews of science must find their point of attachment. Still, I have every reason for confiding in the support of the Argentine people and government, which have never yet failed to respond favorably to any of my appeals. At the present moment the country is suffering from the severest commercial crisis which it has ever experienced; its treasury is depleted by last year's civil war, the agricultural industries of the whole land have been blighted by the ravages of locusts, and an almost unexampled drought; yet although almost all other appropriations have been cut down, those for scientific purposes have been maintained intact, and all which I thought it right to ask for the Observatory has been voted by the Congress.

Now, first of all, I hope to prepare the manuscript of the Uranometry, for which the charts are already begun; and while this is going forward I hope to prosecute various other subsidiary work. The reduction of the catalogue-observations is so well forward that within a couple of months after I shall have completed the recomputation of the circumpolar list (already far advanced), I think that the current observations for the catalogue may be kept reduced up to date. And very soon after the manuscript of the Uranometry is completed I think that at least one volume of instrumental observations can be sent to the press.

You will be glad to learn that after all the labor, vexation and expense, which my first photographic efforts had entailed, far better success is attending the second endeavor. The new lens seems to be quite equal to the original one, and on favorable nights we obtain images of stars which cannot be brighter than the $8\frac{1}{2}$ magnitude. Although the clock-work of the Equatorial is not equal to the demands upon it, the mechanical ingenuity and perseverance of the assistants has already done much, and will, I am confident, do yet more toward remedying the defects. And the lathe and tools for working in metals, which I brought out with me last December, have rendered most important service. Mr. Mansfield is now constructing a new governor, which will, we hope, secure a greater uniformity of movement to the telescope. Mr. Heard, who is now engaged on the photographic work, is far more successful than his predecessor in the manipulation both of the telescope and the chemicals, and has already, in spite of many difficulties, obtained about seventy good plates, comprising twenty-four different clusters and many double stars. On one plate there are nearly or quite a hundred stars, in the cluster $\Delta.323$. We have also been experimenting upon *Mars*, and the photographs show the detail of the surface quite clearly, but they are not as well defined as could be wished. I have now sent home orders for new enlarging lenses, for which, and for other photographic conveniences,

the Minister of Public Instruction has placed a small sum of money at my disposal from the contingent fund of his department. When I next write I think it will be possible to give you some encouraging accounts, and description of results in detail. As yet I have had no time to undertake any measurements of the plates, but if my plans do not prove amiss, this will soon be possible. I possess an exquisite micrometer for the purpose, the gift as well as the construction of our friend, Mr. Rutherfurd. I have been endeavoring to procure photographic impressions of *Eurydice*, and its comparison stars, for Dr. Galle's new investigations, but the stars are too faint.

The geographical determinations of various points of South America are going forward as opportunity permits. Since my return I have obtained satisfactory exchanges of signals with Santiago de Chile on two nights, and am waiting an additional exchange before deducing the final results. Messrs. Thome & Bigelow are now resting themselves from long continued labor at the Observatory by a journey of geographical exploration up the river Paraná. They carry with them a prismatic reflecting circle, which I obtained for such purposes from Pistor and Martius, and in the use of which they have already attained such proficiency that their respective time-determinations frequently agree to the tenth of a second. The sidereal clock of the Observatory, being connected with the telegraph line, is made to graduate a time-scale on the registers of both stations, while the observers telegraph the beats of a mean-time chronometer, for which they determine the error immediately before and after these signals. The position of the city of Paraná was thus determined three days ago; to-day we have accomplished the same for La Paz, which is about 100 miles farther up the river. The telegraphic communication only extends as far as Corrientes in $27\frac{1}{2}°$, but with the aid of their chronometer I trust they will be able to extend the longitude determinations as far as Ascencion, the capital of Paraguay, latitude 25° 16′. I have also made arrangements for an early determination of the positions of Tucuman, situated 260 miles north of Córdoba, and of San Juan at the base of the Andes, some 200 miles to the westward. You will remember that distances form no criterion of ease of access. About eight days are usually required for the journey to Tucuman, notwithstanding that the railroad now building is already nominally completed for half the distance, and the same is true in regard to San Juan.

In Dr. Petermann's new map of the Argentine Republic, just published, the results of my previous observations are incorporated, having been furnished him by Mr. Moneta, late Chief of the National Engineers, in connection with whom my determinations of Rosario and Buenos Aires were made, as I once wrote you. Before long I hope to be able to include in this series of determinations all the capitals of provinces, and the chief points of commercial or geographical importance, to which the telegraph extends.

The Meteorological office is quietly extending its influence and activity throughout the country. It is not easy to secure the gratuitous coöperation of competent and conscientious observers, nor to secure their reception of instruments in good condition; yet the success has thus far been all that I could have presumed to hope; and I am already rapidly accumulating data, which will disclose the heretofore unknown climatic and atmospheric relations of this vast country. Already some dozen or more stations are regularly transmitting their tri-daily observations, and arrangements are already in progress which give ground for confident expectation that this number will soon be doubled. Especially I have reason to believe that systematic observations will speedily be organized along the river Uruguay, in Patagonia, Tierra del Fuego, and the Falkland Islands. The most extensive railroad company in the province of Buenos Aires, and the provincial telegraph have promised their coöperation by establishing observations at their chief stations, and the Right Reverend Dr. Stirling, the English missionary Bishop of the Falkland Islands, has cordially interested himself in our undertaking, and will avail himself of the opportunities afforded by his official journeys to all the Anglican missions in this portion of the world to enlist the assistance of his clergy.

Before bringing this letter to a close I must add one more item of interest, viz., that the two Houses of the Argentine Congress have by decided majorities adopted the metric dollar of one and a half grams of pure gold, or one and two-thirds grams of alloy, nine-tenths fine, as the national monetary unit, the legal value of foreign gold coins to be determined on this basis by assays, and annually published by the Minister of Finance. That this unit is not already law is due solely to disagreement between the two houses on other points contained in the same bill; but the delay is only temporary.

Now that Japan and the Argentine Republic have adopted a metric basis for their monetary unit,—the only basis on which we can hope for any approach to international unification,—perhaps we may look forward to some corresponding action on the part of our own government. The United States dollar requires only a change of less than one-third part of one cent, (less than the limits of mint tolerance), and it would seem as though by the united action of our men of science, this desirable change might be effected during the present demonetization of gold. Such an act would be a most notable addition to our series of centennary celebrations, as it would be a most efficient step toward what the French term "the solidarity of nations."

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *An Air Damper for Balances.*—In order to reduce the time of oscillation of a balance to a minimum when in use, Arzberger has proposed the use of a damping apparatus containing air. To the stirrup, immediately under the end of the beam, a circular plate of brass, gilded, is hung by a wire. This plate is 67 mm. in diameter and ½ mm. thick. It swings, when the balance oscillates, in a short cylinder 68 mm. in diameter and thus has a clear annular space of ½ mm. between it and the cylinder. This latter is fastened to the balance case by a strip of brass, to which it is attached by a screw in the center. The bottom of this cylinder has a hole on one side of this screw; so that by rotating the cylinder, the hole may be closed more or less completely by the brass strip. When the balance swings, the plate moves up and down in the cylinder, the oscillation being checked by the resistance of the air in the box, which may be adjusted at pleasure. The working of the attachment is said to be very satisfactory, since the damping is a dynamic and not a static resistance. The delicacy is not at all interfered with.—*Liebig's Annalen*, clxxviii, 382, Oct., 1875. G. F. B.

2. *On a Convenient method of Preparing Sulphuryl Chloride.*—Behrend, in order to investigate the relations of the carbamides and sulphamides, found it necessary to devise some better method for preparing sulphuryl chloride. For this purpose he prepared sulphuryl chlorhydrin by the action of hydrogen chloride upon sulphuric oxide, and distilled this continuously with a return cooler; but with no satisfactory result. It was then placed in sealed tubes and heated for 12 to 14 hours in a paraffin bath to 170°–180°. A brilliant mobile liquid, boiling at 70°, and with an exceedingly irritating odor, was obtained, which had a specific gravity of 1·661 and was the body in question. Its production is thus represented:—

$$\left.\begin{matrix} SO_2 \{ OH \\ \quad\ \{ Cl \end{matrix}\right\} = SO_2Cl_2 + SO_2 \{ OH \\ \quad\ \{ OH.$$

—*Ber. Berl. Chem. Ges.*, viii, 1004, Sept., 1875. G. F. B.

3. *Action of dilute Mineral Acids on Bleaching Powder.*—Kopfer, under Schorlemmer's direction, has studied the action of dilute mineral acids on bleaching powder, with a view to ascertain whether hypochlorous acid is actually evolved as asserted by Gay Lussac and recently denied by Göpner, and if so, in what amount. The bleaching powder was prepared from pure slaked lime and chlorine, and contained 21·46 per cent calcium hypochlorite, 17·69 calcium chloride, 47·52 calcium hydrate, and 13·33 calcium oxide. A solution of 20 grams of this was made and

filtered. Two hundred cubic centimeters of the filtrate were taken and diluted to a liter, and the hypochlorite was determined by Bunsen's method in a portion of the solution. Other portions were then distilled with nitric, with hydrochloric and with sulphuric acids. The best result was obtained with the first of these acids, the chlorine monoxide (hypochlorous oxide) in the filtrate varying from 80·63 to 91·32 per cent of that originally present in the bleaching-powder solution. Hence the author concludes that when bleaching powder is distilled with dilute mineral acids an aqueous solution of almost pure hypochlorous oxide is obtained in amount rising even to 92 per cent of that in the powder. Hence he infers that bleaching powder contains either calcium hypochlorite with calcium chloride, or a compound of the constitution $Ca\begin{cases} OCl \\ Cl \end{cases}$, as proposed by Odling. The liberation of hypochlorous oxide from bleaching powder by chlorine, is strongly confirmatory of the latter view. Chlorine is not set free by the action of hydrochloric acid upon bleaching powder.—*Jour. Chem. Soc.*, II, xiii, 713, August, 1875. G. F. B.

4. *Method for the Quantitative Separation of Ferric Oxide, Alumina, and Phosphoric Acid.*—Having occasion to analyze a minute quantity of a mineral containing ferric oxide, alumina, and phosphoric acid, FLIGHT took the opportunity to examine the various methods in use for the separation of these substances, the results of which are given in his paper. As a result, he was led finally to adopt the following, which gave all desirable accuracy: The solution of the above named substances, which must not contain much free acid, is boiled for two or three hours with an excess of sodium hyposulphite (thiosulphate) and filtered. All the iron with some of the phosphoric acid is in the filtrate; all the alumina with some of the acid, is on the filter. The iron in the filtrate is thrown down by ammonium sulphide and determined as usual. The alumina precipitate is dissolved, and to the solution an excess of sodium hydrate is added, and then barium chloride so long as a precipitate is produced. After standing a few hours, the solution is to be filtered. The phosphoric acid remains on the filter as barium phosphate; the alumina is contained in the filtrate, from which it may be precipitated in the ordinary way and weighed. A little caustic soda added to the wash water prevents the solution of the barium phosphate. The barium phosphate is dissolved off the filter, the barium removed with sulphuric acid, the filtrate mixed with that from the iron sulphide above, and the phosphoric acid determined with magnesia mixture as usual. The author also points out that the corrosive action even of dilute solution of sodium phosphate upon the glass of reagent bottles is considerable and may be a source of error in analysis. This salt cannot be obtained pure by recrystallization in dishes of Berlin porcelain from this cause.—*Jour. Chem. Soc.*, II, xiii, 592, July, 1875. G. F. B.

5. *On the Gases enclosed in Coals.*—THOMAS has made an elaborate investigation of the gases occluded in coal as well as of

those which are evolved from fissures in the mines. The experiments were made upon the bituminous, or "house coals;" the semi-bituminous or steam coals; and the anthracite coals; all from the South Wales basin. In the first series, slices of coal were sawn from the middle of large cubes and a strip cut from the middle of each of these $\frac{3}{4}$ of an inch square and 6 or 8 inches long. This was placed in a hard glass tube and connected with a Sprengel pump. Very little gas—only 2 or 3 cc. per 100 grams of coal—was liberated on exhaustion. The tube was then immersed in boiling water and kept there so long as gas was evolved. Additional quantities of gas were obtained at 200° and even at 300°. Upon analysis, the gases were found to consist of marsh gas, carbon dioxide, nitrogen and oxygen. The bituminous coals yield the least gas, though it contains a large percentage of carbon dioxide. Steam coals come next, their gas containing as much as 87 per cent of marsh gas. Anthracite yields most gas, one specimen giving 600 cc. of gas from 100 grams of coal, at 100°, 1000 cc. at 200°, and 1875 cc. in all at 300°. In composition these gases closely resembled those from steam coals. The gases collected from blowers in the mine as well as from the coal itself by boring, consisted almost entirely of marsh gas, rising in some cases to 97·65 per cent.—*Jour. Chem. Soc.*, II, xiii, Sept., 1875. G. F. B.

6. *On Chrysophanic Acid.*—LIEBERMANN and FISCHER have continued the researches made upon emodin by the first-named chemist, and have extended their investigations to chrysophanic acid. When distilled with zinc dust, it yields methyl-anthracene, as was proved by a minute examination of the properties and reactions of this hydrocarbon. Acetyl-chrysophanic acid was also prepared and analyzed. It contains two acetyl groups. From these data it is clear that chrysophanic acid is dioxymethyl-anthraquinone and that it stands to emodin in the same relation that alizarin stands to purpurin. The authors notice the curious fact that while the madder-root contains anthraquinone-coloring matters which stand to each other in a simple oxidation series, the root of rhubarb contains a similar series, homologous with the derivatives of anthraquinone. Experiments were also made on the amides of chrysophanic acid.—*Ber. Berl. Chem. Ges.*, viii, 1102, Sept., 1675. G. F. B.

7. *On the Quantitative Determination of Vanillin in Vanilla.*—The introduction into commerce of the synthetically prepared vanillin renders some method desirable for its exact estimation. TIEMANN and HAARMANN have proposed a method founded on its aldehydic nature and its consequent union with acid sulphites. Thirty to 50 grams of the finely divided vanilla is repeatedly extracted with ether, the extracts united, the ether distilled nearly off, and the residue treated with an equal volume of a mixture of equal parts of hydro-sodium sulphite solution and water. On standing, two layers of liquid appear, which are separated by means of a separating funnel. The lower, which is the sulphite-vanillin solution, is placed in a flask furnished with funnel and de

livery tubes, through the former of which dilute sulphuric acid is poured, the evolved sulphurous oxide being conducted into a bottle containing soda. After the reaction is over, the liquid is extracted with ether, the ether distilled to a small bulk, the residue placed in a watch glass and allowed to evaporate spontaneously. Pure vanillin, fusing at 81°, crystallizes out; it is dried over sulphuric acid and weighed. After proving the exactness of the method, the authors used it in analyzing the vanillas of commerce. They found Mexican vanilla to yield 1·69 per cent, Bourbon vanilla 2·46 per cent, Java vanilla 2·75 per cent, and a second sample of Bourbon vanilla 1·91 per cent of vanillin. The prices of the various sorts of vanilla are even more discrepant.—*Ber. Berl. Chem. Ges.*, viii, 1115, Sept., 1875. G. F. B.

8. *On the Localization of Arsenic in the Tissues.*—Scolosuboff has investigated in Gautier's laboratory the distribution of the metalloid arsenic in the tissues and has found, contrary to the general belief, that it is specially condensed in the nervous tissues. The experiments were made with dogs, with rabbits, and with frogs. Dogs bear large doses of arsenic readily, being able to take without difficulty 15 to 18 times the quantity which weight for weight would be fatal to man. The arsenic was administered as sodium arsenite in the food. It was separated from the tissues by treating these with nitric mixed with a little sulphuric acid, evaporating nearly to dryness, adding more sulphuric acid, heating again till fumes appeared, then more nitric acid and heating to carbonization. On exhaustion with boiling water and filtering a liquid is obtained from which the arsenic may be precipitated by hydrogen sulphide. A bull dog took for 34 days gradually increasing quantities of arsenic from 5 milligrams to 150 milligrams per day. 100 grams of the muscle contained ·00025 arsenic, 100 grams liver ·00271, 100 grams brain ·00885, and 100 grams spinal cord ·00933 grams. Calling the quantity in 100 grams of muscle 1, that in the same weight of liver is 10·8, brain 36·5 and spinal cord 37·3. The results where the poisoning was acute were quite as marked. A dog of 11 kilograms was killed by the sub-cutaneous injection of sodium arsenite, in 17 hours. The arsenic from the brain gave a decided reaction, that from the cord was less, while for the liver and the muscles, only traces could be detected. The toxical importance of these facts is obvious.—*Bull. Soc. Ch.*, III, xxiv, 124, Aug., 1875. G. F. B.

9. *Cold Bands of Dark Spectra.*—MM. P. Desains and Agmonet have examined with a thermopile the spectrum of the flame of the lamp of Bourbouze and Wiesnegg; this source of heat is more convenient and trustworthy than the Drummond lamp. With a 60° prism and lenses of rock-salt, on passing the light through a centimeter of water, four cold bands were distinctly shown. Their distances from the extreme red end of the spectrum were 19′·8, 30′·6, 42′ and 52′ respectively. These measurements cannot have the same precision as those of the dark lines of the spectrum, but they show very nearly where the pile must be placed to receive the minimum amount of heat.

The pile was placed about 30 cms. from the axis of the prism, and the breadth of the admission slit was half a millimeter; its angular magnitude as seen from the prism was 5′·7, conseqently each cold band made its effect sensible through an angle equal to its own angular width increased by 5′·7. The illumination slit also had a width of half a millimeter.

According to the measurements of M. Lamanski four cold lines in the spectrum of the sun are distant from the extreme red by the angles 19′·1, 30′, 44′, and 51′. These positions are evidently the same as those of the lines given above by the absorption of a centimeter of water. This coincidence seems to prove that the cold bands in the dark portion of the solar spectrum are in a great measure due to the water of the atmosphere.

Another series of experiments were made to compare the action exerted on dark spectra by different solutions, formed by disolving a solid capable of forming cold lines, in liquids nearly inactive in this respect. The active substance employed was iodine, which was dissolved in chloride of carbon, chloroform and sulphide of carbon. These three liquids dissolve iodine in large quantities, and all three solutions have the same appearance. A layer of liquid, one centimeter in thickness, was used and the prism and lenses were of flint glass. Three lines were observed having the positions given below:

Chloride.	Chloroform.	Sulphide.
1° 28′	1° 30′	----
1° 34′	----	1° 35′
1° 55′	1° 57′	1° 56′

These numbers show that the action of the iodine is the same in the three cases. In all the experiments observations were made on that part of the spectrum where the iodine solution produced a line, so that the solvent and the whole refracting system had no action in the production of the phenomenon.— *Comptes Rendus*, lxxxi, 432; *Phil. Mag.*, l, 331. E. C. P.

10. *Luminosity of Flames.*—M. F. Wibel has examined the causes of luminosity and non-luminosity of flames. Knapp found that the flame of a Bunsen's burner became non-luminous if nitrogen, hydrochloric acid, or carbon dioxide be passed into the air holes instead of air. Blockmann found that carbon monoxide and hydrogen have the same action, and the same effect is produced, as Landon has lately shown, by steam. When, however, any such mixture is very strongly heated before it undergoes combustion, it again becomes luminous. This is best shown by fixing a platinum tube into the upper end of the burner and heating it with two horizontal gas-jets. An iron tube may also be used instead of the platinum tube, but, on account of the higher specific heat of iron, it must be more strongly heated. A copper burner cannot be used, because it colors the flame green. The luminous flame appears as a cone between the inner dark cone and the outer bluish cone. This is not caused, as might be supposed, by the gases

of combustion of the two burners surrounding the colorless flame and thus keeping the oxygen away, inasmuch as the luminosity is equally well produced when the access of these gases is prevented by a large sheet of iron having a hole in it, through which the tube of the burner passes air-tight.

From these observations it may be concluded that the non-luminosity of a gas-flame is not caused by a dilution of the gas, this dilution being in fact increased by heating the mixture. The only cause is a cooling of the interior of the flame. This is further proved by the fact that it is most difficult to get a non-luminous flame from a mixture of coal-gas and oxygen, showing that neither rapid oxidation nor dilution produces the non-luminosity.—*Deut. Chem. Ges. Ber.*, viii, 226; *Jour. Chem. Soc.*, xiii, 603.

E. C. P.

11. *Rotatory Polarization of Quartz.*—MM. J. L. Soret and E. Sarazin have repeated the experiments of Broch and Stefan extending them to the more refrangible portions of the spectrum. The light of the sun concentrated by a lens 1·15 m. focus and 72 mm. in diameter, was passed through two Nicol prisms with a plate of left-handed quartz 30·085 mms. thick between them. It was then observed with the fluorescent eye-piece of M. Soret. For the rays *A* and *a*, a piece of cobalt glass was interposed to cut off the adjacent bright light.

	λ	ϕ	Δ
A	760·0	12°·68	+0·10
a	718·5	14·33	·00
B	686·7	15·75	·00
C	656·2	17·35	·00
D	588·9	21·79	−·05
E	526·9	27·61	−·06
F	486·07	32·85	−·07
G	430·72	42·63	+·06
h	410·1	47·52	−·05
H	396·8	51·22	·00
L	381·9	55·88	−·08
M	372·7	59·03	−·06
—	372·0	59·24	·04
N	358·5	64·41	+·03

The formula $\varphi = A + \frac{B}{\lambda^2}$ in which φ is the angle of rotation and λ the wave-length agrees well with observations between *B* and *H*, but is inexact if used between more extended limits. If the constants are computed for *a* and *M* the value for G is erroneous by a whole degree. Starting with the idea that the rotation ought to be nothing for a wave of infinite length, M. Boltzmann has proposed the formula $\varphi = \frac{B}{\lambda^2} + \frac{C}{\lambda^4} + \frac{D}{\lambda^6} +$ &c. Retaining two terms only, and computing *B* and *C* for the lines *a* and *M* gives the formula, $\varphi = \frac{7{\cdot}10533}{10^6\lambda^2} + \frac{0{\cdot}151227}{10^{12}\lambda^4}$ which agrees well with

observation. In the annexed table, column I. gives the name of the various lines, column II. their wave-lengths, column III. their observed angle of rotation, each being the mean of from six to forty-six observations; the last column gives the difference between column III. and the value computed by the formula given above.—*Comptes Rendus*, lxxxi, 610. E. C. P.

12. *Researches on the Hexatomic Compounds of Cobalt;* by Wolcott Gibbs, M.D. 51 pp. 8vo. From the Proceedings of the American Academy of Arts and Sciences, vol. xi.—The earlier parts of Dr. Gibbs' researches on the cobaltamines have been published in this Journal, and will be found as follows: III, vol. vi, p. 116; also vol. viii, p. 189 and p. 284. For the conclusion of the memoir the chemical reader is referred to the article of which the title is given above; notwithstanding its great value, the length is so considerable as to make the republication in this Journal hardly possible.

13. *Professor Tyndall and Professor Henry on Sound.*—The following criticisms are here cited from a notice of the new edition of Prof. Tyndall's work on Sound (1875), in "The Nation" for October 28:

"The reader who turns to this seventh chapter will find that it opens with an 'introduction' professing to give 'a summary of existing knowledge' in the matter of fog-signalling. The writer states that while the *velocity* of sound has formed the subject of repeated and refined experiments by the ablest philosophers, 'the publication or Dr. Derham's celebrated papers in the Philosophical Transactions for 1708 *makes the latest systematic inquiry into the causes which affect the intensity of sound in the atmosphere.*' And, after making this statement, the Professor immediately adds as follows: 'Jointly with the Elder Brethren of the Trinity House, and as their scientific adviser, I have recently had the honor of conducting an inquiry designed to fill *the blank here indicated.*' In order still further to impress on the reader a sense of the magnitude of this 'blank,' Dr. Tyndall indulges in one or two preliminary references which, he says, 'will suffice to show the state of the question when this [his] investigation began.' The first of these references cites the opinion of Sir John Herschel to the effect that fogs and falling rain, and more especially snow, had been found by Derham 'to tend powerfully to obstruct the propagation of sound.' The second of his references is made to what he calls 'a very clear and able letter' addressed by Dr. Robinson of Armagh to the British Board of Trade in 1863. In this 'very clear and able letter' Dr. Robinson states that sound is the only known means for coping with fogs, but about it, the 'testimonies are conflicting, *and there is scarcely one fact relating to its use as a signal which can be considered as established.*' But Dr. Robinson is clear on one point—to wit, than 'fog is a powerful damper of sound.'

On the strength of these historical references, Dr. Tyndall ventures the remark that, prior to the investigation conducted by

him, the views enunciated under this head by Derham, Herschel, and Robinson 'were those universally entertained.' It was in order to fill 'the blank' indicated by the universal prevalence of such erroneous opinions that *his* inquiry, he says, was set on foot. And his inquiry, he tells us, was begun on the 19th of May, 1873.

Now, it is a matter not only of scientific knowledge, but of public notoriety in this country, that extensive researches on 'the cause which affect the intensity of sound in the atmosphere' had been made by the United States Light-House Board long before Prof. Tyndall began his investigations. That he should have chosen to ignore the fact in the body of his present volume becomes only the more surprising when, on turning to its preface, we find that he was, as he confesses, 'quite aware *in a general way* that labors, like those now for the first time made public, had been conducted in the United States,' and 'this knowledge,' he subjoins, 'was not without influence upon my conduct.' If his knowledge of the similar labors conducted under this head in the United States was not, as he acknowledges, without influence on his conduct in giving direction to his researches, it will naturally occur to ordinary minds that this knowledge should also have been 'not without influence' on his pen when he was professing to give a summary of the existing state of science on this subject. And when to this statement of the case, as acknowledged by himself, we add that he was made acquainted with the nature and purport of Prof. Henry's explorations on this question, not only 'in a general way,' but also in a very special way, it becomes still more inexplicable that in defining 'the blank' which he claims to have filled by his recent inquiry, he should have disregarded the labors and results of American science, and that too while profiting by the instruments and methods of that science in the very conduct of his investigations. The reader will understand the force of our remark that Prof. Tyndall was acquainted with the researches of Prof. Henry, not only 'in a general way,' but also in a special way, when we state that a paper by the latter—on the abnormal phemomena of sound in relation to fog-signalling—was read by its author in the hearing of Prof. Tyndall at a meeting of the Washington Philosophical Society, called for the purpose of doing honor to the British savant while he was sojourning in the national capital. And the force of our remark that he has ignored the results of American science in magnifying 'the blank' which he describes, while profiting by the instruments and methods of that science in conducting his inquiry, will be understood when we say that the researches of Prof. Tyndall were prosecuted with the help of a steam-syren, gratuitously lent to him by the Light-House Board at Washington, constructed and patented by a citizen of New York, and introduced by Prof. Henry into the light-house system of the United States." * * *

"It is no part of our present purpose to institute a critical inquiry into the conflicting views of Prof. Henry and of Prof. Tyndall with regard to the hypotheses respectively espoused by

each for the explanation of the phenomena of sound in its passage through wide tracts of air. Prof. Henry believes that the direction and the rate of wind-currents are important elements in the problems presented by the phenomena in question. Prof. Tyndall admits that 'the well-known effect of the wind is exceedingly difficult to explain,' but he insists on making up the fagot of his scientific opinions on the subject at once and for ever without taking the 'viewless winds' into his account. He finds a sufficient explanation of all the abnormal phenomena in the assumption of ideal clouds of vapor mingling with the atmosphere so as to disturb its homogeneity, and thereby quench the body of sound. There is nothing in the working hypothesis of Prof. Henry which excludes any truth there may be in the working hypothesis of Prof. Tyndall. But in the present provisional state of his inquiries on the subject, the former is disposed to question the sufficiency of the explanation adduced by the latter as an efficient cause of all the phenomena in question. With the modesty and reserve of the true physical philosopher, in the present unfinished state of scientific inquiry, Prof. Henry waits for the wider knowledge which shall furnish the basis of an assured induction meeting all the requirements of the problem."

14. *A new Treatise on Elements of Mechanics, establishing strict precision in the meaning of Dynamical terms, accompanied with an Appendix on Duodenal Arithmetic and Metrology;* by JOHN W. NYSTROM, C.E. 352 pp. 8vo. Philadelphia, 1875. (Porter & Coates.)—This volume contains a great number of problems in practical mechanics, with concise and excellent solutions, and some useful tables. The author makes some innovations in the technical language of the science, which are not likely to be considered as in all respects improvements, and some of the topics treated in the volume are rather irrelevant to the subject. A. W.

II. GEOLOGY AND NATURAL HISTORY.

1. *Primordial fossils in pebbles of the Newport (Rhode Island) Conglomerate.*—Prof. WM. B. ROGERS, in the Proceedings of the Boston Society of Natural History for 1875, p. 97, discusses the origin of the flattened form of the siliceous pebbles or stones of the Newport conglomerate, and attributes it to sea-shore attrition. He also announces the discovery of many stones from the beach, that were derived from the conglomerate, which are "crowded with well-preserved impressions" of the Potsdam *Lingula*, similar to those found by him, several years since, in masses of the same conglomerate at Fall River, 17 miles to the east of north of Newport. Some impressions of the same fossil, less distinct, he has observed in the rock in place at Newport.

2. *Bowlder near Batavia, Genesee County, New York.*—The bowlder "on the road from Caledonia to Batavia," figured by Hall on page 341 of his Report on New York Geology (1843), and stated to consist of Hydraulic limestone from the upper part of the

"Onondaga Salt group," has the following dimensions, according to a recent letter to one of the editors received, along with an excellent stereoscopic photograph, from Mr. J. G. Fargo of Batavia: height 12 feet; width of base 20 feet, of neck 8 feet, of top 14 feet. It is underlaid by about 40 feet of drift, beneath which is the Corniferous limestone.

3. *On a method of distinguishing the different Feldspars by means of their optical properties;* (Extract from a letter from M. DESCLOIZEAUX, dated Villen-sur-mer, July, 3d, 1875.)— * * * This method can be applied to all the feldspars, and gives a much easier means of distinguishing them than those described in my last memoir (this Journal, III, ix, 322). It is only necessary to obtain a section, sufficiently thin and transparent, parallel to the easiest cleavage [p] and smooth enough to be homogeneous in all its parts. Similar sections obtained from crystals or lamellar masses of albite, oligoclase, labradorite, and the majority of those of microcline, show hemitropic bands, more or less close together, arranged along the plane parallel to the second cleavage [g^1]; for orthoclase and microcline in *simple crystals*, two sections placed in opposite directions serve to produce the same effect. These sections are thus brought between the crossed Nicols of a polarization-microscope.

(1.) For *orthoclase* the maximum extinction takes place when the two sections are parallel to their plane of contact; the edge $p\,g^1$ being in the plane of polarization of the microscope.

(2.) For *microcline*, the whole structure consists of a multitude of very fine parallel bands; we have microline alone, either hemitropic or not hemitropic, or microcline and orthoclase; the extinction can take place at 30° 54′ between the adjoining bands of the same plate of the macle (microcline alone), at 30° 54′ between the two plates of the macle (microcline in bands), or at 19° 27′ between the adjoining bands (microcline and orthoclase.) In the last case the whole of two lamellæ of the macle show at the same time an extinction oblique to the plane of composition, belonging to the microcline, and one parallel to this plane for the orthoclase.

(3.) For *albite*, the extinction between two bands takes place at an angle of 6° 32′.

(4.) For *oligoclase*, the extinction is simultaneous in the two bands, and when the plane of composition coincides with the plane of polarization of the polariscope, it shows that the structure is homogeneous.

(5.) For *labradorite*, the extinction takes place at 10° 24′ between the alternate lines of the hemitropic lamellæ.

It follows from this that a plane normal to the plane of the axes cuts the base along a line making with the edge $p|g^1$ the following angles:

0° in orthoclase and microcline.
19° 27′ in microcline.
3° 16′ in albite.
7° 12′ in labradorite.

I shall determine the same angle for anorthite when I return to Paris. * * * . E. S. D.

4. *Contribuzoni Mineralogiche per servire alla Storia dell' Incendio Vesuviano del Mese di Aprile*, 1872, *memoria di* ARCANGELO SCACCHI. Parte seconda, 69 pp. quarto. Naples, 1874.— The first part of Scacchi's valuable memoir on the eruption of Vesuvius in the Spring of 1872 was published two years since. The second part includes descriptions, mostly crystallographic but also to some extent chemical, of the following species, produced by the eruption: hematite and magnetite, tenorite (melaconite), *atelite*, sylvite and halite, sal ammoniac and *cryptohalite*, *chlorocalcite*, cotunnite and *pseudocotunnite*, *erythrosiderite*, molisite, *chloromagnesite*, *chloralluminite*, aphthitalite, anhydrite, *cupromagnesite*, *chlorothionite*, *microsommite*, pyroxene, byssolite, *hydrofluorite*, *proidonite*, chrysolite, apatite. The names of the new species are printed in italics; their principal characters are given below, with the exception of several already described in Appendix II, to Dana's Mineralogy, 1875.

Atelite; produced by the action of hydrochloric acid on the lamellar crystals of tenorite (melaconite), by which their color is changed from black to green. Its composition is expressed by the formula $2CuO, CuCl+3HO$.

Cryptohalite; a fluosilicate of ammonia, associated with sal ammoniac, and having the composition $2NH^4Fl, SiFl^2$.

Pseudocotunnite; occurs with cotunnite in acicular yellow crystals, opaque and lusterless. In water they lose their color, dissolve in part, and the remainder separates as a white powder. Composition, probably expressed by the formula PbCl, KaCl.

Chloralluminite; composition Al^2Cl^3, xHO, associated with molisite, and chloromagnesite; decomposes on exposure to the air.

Chlorothionite; found in crusts of an azure-blue color, an analysis of which agreed approximately with the formula Ka, Cu, Cl, So^4.

Hydrofluorite; hydrofluoric acid, HFl; proidonite, $SiFl^2$; both products of the eruption of 1872. E. S. D.

5. *Materialien zur Mineralogie Russlands;* von N. VON KOKSCHAROW. Vol. vi, pp. 209-407, with index, vol. vii, pp. 1–176, 8vo. St. Petersburg, 1874, 1875 The crystallographic researches of M. von Kokscharow are always remarkable for their thoroughness and great exactness. The volumes, or better parts of volumes, last published, include memoirs upon a large number of species, of which the following are some of the more important: native copper, vi, p. 209; aragonite, p. 261; dioptase, p. 285; scorodite, p. 307; gold, p. 321; sulphur, p. 368; perofskite, p. 388. Dolomite, vol. vii, 1; barite, p. 25; calcite, p. 59; crocoite, p. 97. E. S. D.

6. *Memoire sur la Famille des Pomacées;* par J. DECAISNE.— A detailed analysis of Decaisne's monograph of the genus *Pyrus* was given in this Journal (3rd ser., iv, 489, Dec., 1872). Some of the views taken in that work are fully expounded in the present paper, which is separately issued from the Archives du Museum,

x, pp. 113–192, tab. 8–15, imp. 4to. The results of a prolonged study of an important group, by a botanist of great experience and ability, is worthy of particular attention. As the veteran author states it:—

"My principal object is here to call the attention of botanists to certain characters which have been neglected in systematic works, by the aid of which the ancient genera [merged in *Pyrus* by most of the recent systematists] may be neatly circumscribed. Such is the constancy and the value of these characters that the details of organization peculiar to each generic group may be expressed by very general propositions, which is the very object of a good method. Indeed, when a special organization is common to a large number of different plants, it is evident that comparatively slight but constant modifications of this structure ought to be particularly attended to; and this proposition seems to be especially true of the *Pomaceæ*." M. Decaisne puts foremost his strongest point when he declares of the Quince, that "the nature of its bark and wood, its prefoliation, inflorescence, the æstivation of the corolla, the structure of the ovary and of the fruit differ essentially from that of the Pears, among which certain botanists still class it." Rather than combine the Quince and the Japan Quince with *Pyrus*, we are confident that botanists will generally accept his *Docynia*, along with *Chœnomeles* Lindl. and *Cydonia*, as independent genera. The same may be said of *Mespilus;* and it must be allowed that the character which Kunth had noticed and which Decaisne has turned to account, that of the deformation of one of the ovules which becomes a kind of stipitate hood for the other, being common to it and to *Cratægus*, indicates a relationship to the latter genus rather than to *Pyrus*. Much nicer and more questionable characters are assigned to the genera here re-established from *Pyrus* in the Candollean sense, to which we are in this generation accustomed. These are, *Aronia* Pers., our Chokeberry (in which eight species are set up from what we take to be a single polymorphous one); *Sorbus* Tourn., the Mountain Ash (the synonym *S. microcarpa* omitted from *S. Americana*, and *S. sambucifolia* is still taken to belong only to our western coast, whereas it extends across the continent); *Aria* Host., the Beam Trees, all of the Old World; *Torminaria* Roem., for *Pyrus torminalis;* also *Cormus* Spach, for *Sorbus domestica* L., the Service-tree of Europe, with *Pyrus trilobata* DC., and an allied species; *Micromeles*, a new genus for four Himalayan species thus far little known; lastly *Malus* Tourn. Here it is to be observed that *M. diversifolia* is held to be distinct from *M. rivularis;* and that a subgenus, *Chloromeles*, proposed for *M. angustifolia*, our narrow-leaved Crab-Apple, thus widely separated from *M. coronaria*, on account, as is stated of its reddish anthers and the structure of the disk. *Pyrus* Tourn., is thus brought down to the Pear; and this, as Decaisne had formerly announced, to a single collective species, of six geographical *proles* or forms. We continue to write *Pyrus* from old habit and custom, not doubting, however, that *Pirus* is the correct orthography.

Of *Amelanchier*, following Lindley, there are enumerated twelve species, six for the Old World and six for North America, and there are names for four more. Without being able to clear them up (and no wonder), Decaisne thinks that they may be distinguished into at least three groups, characterized by the distinct or united styles, and the glabrous or downy ovaries. We are continually impressed with the idea that there must be three or four American species, and the seeds may aid in their definition. But thus far the characters elude investigation. *Peraphyllum* Nutt., referred by Bentham and Hooker to *Amelanchier*, has not been studied by Decaisne. When he examines the excellent specimens in flower and in fruit, which Mr. Siler has supplied from Southern Utah, he will conclude that the genus must certainly be reinstated. The likeness is only in the peculiar structure of the fruit.

As respects the remaining genera, the difference between this monograph and the disposition in Bentham and Hooker's Genera Plantarum is mainly this: *Eriobotrya*, with its baccate fruit (what is termed endocarp reduced to a soft pellicle), large turgid seeds with thickened cotyledons, and undulate petals, is upheld as a good genus: *Heteromeles* is adopted from J. Roemer for the Californian *Photinia arbutifolia*, and a second (probably not good) species, *H. Fremontiana*, is added. The characters appear to be the 10 instead of 20 stamens, in pairs opposite the calyx-lobes, their filaments dilated at base and somewhat monadelphous. In the tabular conspectus the petals are said to have "préfloraison tordue," but in the generic character it is "æstivatione imbricativa vel convolutiva," the latter term with the French botanists meaning the same as imbricated only more enrolling. The diagram represents the whole five petals with one edge covered, i. e., "tordue" or contorted (or, as we say, convolute), and so we find them in all the flower-buds now examined. But before adopting the genus it may be well to examine the *Photiniæ* generally. *Photinia*, of which *P. serrulata* is the type, is characterized as having imbricative æstivation, and Decaisne's diagram represents it as regularly (i. e., quincuncially) so. But in *P. prunifolia* and in *P. Blumei* we find occasionally only one exterior petal, and the four others successively overlapping in the "contorted" way; and in one of Wallich's specimens of *P. integrifolia* the first flower-bud inspected showed the "contorted" æstivation complete. This is also the case in *P. dubia* (in one of Hooker's and Thompson's Khasyia specimens), and this Decaisne refers to *Eriobotrya*, which has imbricative æstivation. Next is *Pourthia*, a new genus of eleven Japanese and Indian species, the type being *Photinia arguta*, *villosa*, *lævis*, &c., and the character, among others, "æstivatione contorta." But we as commonly find one petal wholly exterior. So we think it evident that the æstivation of the corolla furnishes no characters for the division of the genus *Photinia*.

Finally, as to the proper stone-fruited genera, *Pyracantha* is adopted from J. Roemer, for *Cratægus Pyracantha* and an allied Indo-Chinese species, and placed near *Cotoneaster;* and a character

not before used is introduced, namely, the position of the cotyledons, which, in this genus are, as regards the rhaphe, accumbent.

There are eight plates, six of them filled with admirable dissections, neatly done upon stone by Riocreux from the author's sketches. A. G.

7. *Elementary Lessons in Botanical Geography;* by J. G. Baker, F.L.S.—These first appeared as articles in the Gardeners' Chronicle, where they attracted attention as interesting and very readable sketches of the leading facts respecting the geographical distribution of plants, and the elements of climate which control this distribution. It appears that they were written by Mr. Baker, of the Kew Herbarium, and Lecturer on Botany at the London Hospital. Now collected, they form a neat 18mo volume, of 110 pages, a sort of "science primer," published by Lovell Reeve & Co., London, 1875. A. G.

III. Astronomy.

1. *Observations on the late Transit of Venus at Beechworth, Victoria;* by Dr. Henry J. Anderson.—The following interesting description of the observation of the late Transit of Venus, at Beechworth, Victoria, by Dr. Henry J. Anderson of this College, is taken from a letter addressed to me which has just been received and which bears the date Srinagar, Cashmere, India, August 30th, 1875. The telescope used has a clear aperture of 77 millimeters and a focal length of 1·125 meters. It was made by Secretan of Paris.

"The telescope was subjected, for this purpose, to chosen tests by Mr. Lewis Rutherfurd, then in Paris, and always so ready to oblige. I used the lower magnifying power, 98 I believe. Various circumstances interfered with my obtaining an observation of the exact moment of the first external contact. I did not see the planet until at least twenty degrees of its limb was on the sun's disc. This occurred at $11^h\ 31^m\ 40^s$, Melbourne time, as sent by telegraph from Melbourne to Beechworth where I observed. Thirteen seconds had probably elapsed. At $11^h\ 35^m\ 30^s$ the interior tangents to the two obtuse cusps were at right angles to each other. At $11^h\ 45^m\ 29^s$ these tangents were to all appearance parallel. At $12^h\ 0^m\ 9^s$ the cusps (then singularly sharp and well-defined) were distant from each other about one-seventh of the diameter of the planet. The atmosphere at this time was all that could be desired. A remarkable calm and clearness of the sky prevailed. There was no cloud, or even fleeciness in the direction of the sun, which was then only fifteen degrees from the zenith. As the observation was made with a diagonal eye-piece, the great altitude of the sun was very favorable to steadiness and distinctness of view, both from the position of the observer and the condition of the atmosphere. The instrument was adjusted as to focus, etc., as nearly as possible in accordance with the recommendation of Messrs. Andrè and Wolf, and whatever may have been the cause, nothing in any way resembling the "black drop" made its appearance, either as the first internal contact drew nigh

or at any other time. The cusps approached each other with a motion geometrically, and to me surprisingly, precise; without bluntness, flickering or vibration. Such portions of the planetary disc as were near or between the cusps, seemed visibly less dark than the main portions more remote; but this gradation of hue was limited in extent and apparently without influence on the formation of the luminous thread.

Just preceding the union of the cusps the approaching points seemed to lose for a second or two the regular precision of their motion toward each other; but the filament once formed did not appear to me to break again. This phase occurred at Beechworth [longitude 146° 43′ 0″ = 9^h 46^m 52^s E. of Greenwich, latitude 36° 21′ 40″ S.; height above sea level 1800 feet] at 11^h 0^m 24^s·6 Melbourne mean time; subject to any error, against which all possible precautions were taken, either in the telegraph from Melbourne or in the chronometer carried from the telegraph office to the place of observation.

Not many minutes had elapsed after the first internal contact when a sudden change of weather ensued. A high wind sprang up and dense clouds, at intervals, quite obscured the sun. At two P. M. the planet ceased to be visible and no phase of the egress could be observed. I wish to express my warmest thanks to Mr. William J. Turner, not only for the use of his grounds, but for his skillful aid in securing for me a steady manipulation of the telescope and its appendages.

The longitude and perhaps the latitude of the place of observation, no doubt need some slight correction."

L. Waldo.

Columbia College Observatory, N. Y., Oct. 16th, 1875.

IV. Miscellaneous Scientific Intelligence.

1. *The Anderson School;* letter to the Editors of the Evening Post from Prof. Alexander Agassiz.—A letter signed "E," lately printed in the Evening Post, contains certain charges against the trustees of the Anderson School. Before answering them the position of the director and trustees toward Mr. Anderson may be briefly noticed.

It may or may not have been known to Mr. Anderson that only the urgent solicitation of Professor Agassiz overcame my reluctance to serve as one of the original trustees of the school at Penikese. My appointment as his successor was against my own wish, and of this Mr. Anderson was fully aware. It was always agreed between Professor Agassiz and myself—and the founding of a summer school in no wise changed this understanding—that as his scientific executor the care of the museum at Cambridge should be my special charge, and that toward its support alone whatever I could contribute personally or command from others would be applied.

I held the nomination as director of the Anderson School, left in writing by Professor Agassiz, in abeyance, until early in 1874 the opportunity occurred to confer with Mr. Anderson, when I ex-

plained to him my position with reference to the museum at Cambridge. He, therefore, knew that, should this nomination be accepted, the duties of the director of the school must remain subordinate to those of the director of the museum, and that the interests of the former would not be allowed to interfere with the prospects of the latter. With this reservation, and the condition that Mr. Anderson would contribute the sum of $10,000 towards the support of Penikese for the next three years, I agreed to serve. This sum was considered as probably sufficient to carry on the undertaking until the future of the museum was assured. Of this $10,000 Mr. Anderson subsequently forwarded $1,329.60 (the check mentioned by your correspondent to the trustees), notifying them at the same time that for "reasons satisfactory to his own mind" he declined to make any further pecuniary sacrifices for the school. Neither the director nor the trustees could assume the responsibility thus left to them.

With this explanation I may now take up the charges of your correspondent. He says it was "authoritatively stated" that $10,000 of the Anderson Fund remained unexpended, and would suffice for the sessions of 1874 and 1875. No such statement was made, and no such sum existed. With the sanction of Mr. Anderson, the bulk of the "nucleus of an endowment fund" had been devoted by Professor Agassiz not only to buildings and equipment, but also to the running expenses of 1873. It was found, before the opening of the session of 1874, that these expenditures amounted to $47,000. There remained from the Anderson Fund not the $10,000 which figures so largely in the letter of "E," but about $1,100 available to the trustees. With this and $1,500 realized from the yacht Sprite, sold with the consent of Mr. Galloupe, the school reopened. As far as the interests of education were concerned the second session was a decided success, thanks to the interest taken by the teachers; but the institution remained in debt, and Mr. Anderson, to whom an informal account was sent, contributed, as above stated, $1,329.60 to pay the "actual deficit." This left the trustees to provide for existing liabilities, or be reproached, as they now are, with "needlessly sacrificing" the property under their charge. No appeal was made to Mr. Anderson beyond what he had himself proposed to do for the school. It is not true that he was "permanently laid under contribution—under penalty." It is not true that the condition of the school was kept back from him, or that the trustees had not the courtesy to signify to Mr. Anderson their intention of closing it. He has at all times been kept fully informed, partly by letters and partly in conversation, of its condition and prospects, as far as they were known to the trustees themselves.

They made every effort to save the school, to put it on a safe foundation, to make it self-supporting. A guaranty fund of $3,000 was contributed by a member of Professor Agassiz's family, and a very slight permanent support from the public would have enabled them to continue an institution which they close with deep regret. Receiving no such support they decided to wind up the

affairs of the school, a contingency of which Mr. Anderson was duly notified early in 1875, after he had of his own accord dissolved the conditions upon which alone the present director consented to serve. This brings me to the suggestion, made in the form of an accusation, that the trustees of the Anderson School, and by implication the trustees of the museum, might have appropriated a small portion from the Teachers' and Pupils' Fund of the Agassiz Memorial and have applied it, in "better faith and better morals" to paying the debts of Penikese. Neither the trustees of the Anderson School nor the trustees of the museum have any control over the Teachers' Fund. It was raised with the understanding, plainly stated in the circulars issued at the time, that it would be devoted to carrying out the plans of Professor Agassiz for the museum at Cambridge. That this will be done with the full appreciation of the relations which existed between him and those who have borne such affectionate testimony to his memory, every teacher throughout the country may be sure. But the use suggested by your correspondent would have been a strange misappropriation of money contributed for a special purpose.

As to the charge which seems directed against the trustees of the museum, as well those of the Anderson School, that they have trodden on the memory of their former director, the fact that these boards include several of Professor Agassiz's warmest friends, two of his favorite pupils, his brother-in-law, his son-in-law and his son, is a sufficient refutation of this calumny.

Finally, no insults have ever been offered to Mr. Anderson by the trustees, unless their decision not to become indefinitely responsible for the expenses of the school, and to close an institution which no one but themselves seemed inclined to support, can be so construed.

Cambridge, October 12, 1875.

2. *Annual Report of the Board of Regents of the Smithsonian Institution for the year* 1874. 416 pp. 8vo. Washington.—In addition to the report of the progress of the Institution in its various branches, the present volume includes the usual Appendix of Scientific Memoirs. The Memoirs in this new volume are: Eulogies on Laplace, Quetelet and Auguste De La Rive; on tides and tidal action in harbors, by Prof. J. E. Hilgard; Observations upon the electricity of the atmosphere and the aurora borealis, made during the Swedish expedition of 1868 to the north pole, by Prof. Selim Lemström of Helsingfors; On a dominant language for science, by Alphonse de Candolle; On underground temperature, by C. A. Schott; On a series of earthquakes in North Carolina, commencing Feb. 10th, 1874, by Prof. Warren du Pré; on the transactions of the Society of Physics and Natural History of Geneva, from June, 1872, to June, 1873, by A. De La Rive; On warming and ventilation, by A. Morin.

Under the head of ETHNOLOGY, has been collected a series of valuable observations, comprising an account of ancient graves of California, by Paul Schumacher; also descriptions of various

antiquities from Illinois, Ohio, Kentucky, Lake Superior, Mississippi, Tennesse, New York, Indiana, Maryland, North Carolina, and Florida.

3. *Croll on Climate and Time.* — Professor Croll's work on Climate and Time, noticed on page 222 of this volume, has been republished by Messrs. Appleton & Co. of New York.

4. *A Course in Descriptive Geometry for the use of Colleges and Scientific Schools ;* by WILLIAM WALTER, Ph.D. (Boston, L. Prang & Co.)—In this work the treatment is concise without serious omissions or inadequate explanations, and yet sufficiently full without verbosity. The fact that a text of fifty pages requires a table of contents of eleven pages, shows how little needless description is given. Doubtless much of the saving of space is due to the convenient and expressive notation adopted.

The introduction of a series of stereoscopic views of many of the problems is a novel feature, and will doubtless materially aid the student in the place of models. It must not be forgotten, however, that a large part of the value of Descriptive Geometry arises from its training the student to conceive of objects from their projections; hence the value of any such aid, except to initiate the student in his work may be questionable. The collection would have been improved by the addition of one or two more warped surfaces, especially the hyperbolic paraboloid and conoids, which always give trouble to the student. The typographical execution of the work is excellent and the drawings accurate and easily understood. E. C. P.

5. *American Naturalist.* — The American Naturalist will be published the coming year by Messrs H. O. Houghton & Co., Cambridge, Mass., and will appear in new typographical dress. The Journal has hitherto been well conducted, always able in its articles, and while popular in its aim, never popular at the expense of its science. It is announced that the Naturalist will be improved under its new publishers by fuller notices of current science, and in other ways.

6. *School of Geology in the University of Syracuse.*—Under Professor Winchell a school of Geology has been organized in connection with the Syracuse University. It includes a course of Elementary Study or Undergraduate Course, and two Graduate Courses, one Lithological and the other Paleontological, carried on together. There are also to be special studies in different departments of Zoology, as in that of Corals, Brachiopods, Trilobites, &c. The school is intended for special geological students, and also for those who desire a general acquaintance with the subject. The school will open on the 25th of next January. The prospectus announces that Prof. Winchell will be aided in the school by Prof. J. J. Brown in Chemistry, Prof. F. Smalley and Rev. S. R. Calthrop in Geology, Profs. B. G. Wilder and Smalley in Zoology, Profs. James Hall, R. P. Whitfield and E. D. Cope in Paleontology. The position of the school in central New York is particularly favorable for the geological and paleontological student.

THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[THIRD SERIES.]

ART. LIX.—*The Solar Atmosphere, an introduction to an account of researches made at the Allegheny Observatory;* by S. P. LANGLEY, Director of the Observatory.*

IT has long been observed that the sun is not everywhere equally bright, but that the edge is darker than the center, and since a self-luminous sphere should appear of sensibly uniform brilliancy, so as to present to the eye the appearance of a flat disc, this diminution of light must be due to some medium external to the photosphere. Accordingly, even before the invention of the spectroscope, it was admitted that an absorbing atmosphere surrounded the sun, and that the effect would necessarily be to diminish the amount of radiation. La Place, in the tenth book of the Mécanique Celeste, has attempted to compute the total effect of this absorption, from data furnished by the observations of Bouguer, and considers that, were the solar atmosphere removed, the heat and light would be twelve times as great as at present. Though this value is erroneous, being deduced from imperfect data by processes which rest on a false hypothesis, it may yet serve to call attention to the importance of a somewhat neglected field of research, in which this Observatory has been in part occupied during the past three years.

Several estimates of the absorptive power of this atmosphere have been made, differing widely from each other. According to that of La Place, just cited, the absorption is about $\frac{1}{1\frac{1}{2}}$ of the sun's emission; according to Liais less than $\frac{1}{1\frac{1}{2}}$; according to Secchi ·88. These discrepant results may be due in part to different hypotheses used in computation, but they are in every

* Paper read at the Detroit meeting of the American Association for the Advancement of Science.

case founded on an experimental comparison of the light or heat of the sun observed at the center with that observed near the edge.

This direct comparison of the two lights involves, of course, no hypothesis, but is simply a photometric measurement, and apparently an easy one. Let us examine the result of this comparison at different hands. Arago,* using the Rochon prism and analyzer, announced, as the conclusion of a prolonged research, that the light at the center of the solar disc must be diminished by $\frac{1}{42}$ part, or 2·4 per cent, to equal that of the edge. Liais in a memoir † which is a model of conscientious care, announces, from comparisons carried on by the extinction of one light by a stronger, that the central light should be diminished by about 10 per cent, to equal that of the edge. His result is then four times as great as Arago's. Secchi with a wheel photometer, finds, for a nearly corresponding point, 78 per cent as the amount by which the central light should be diminished. This result is then *seven* times that of Liais and *thirty* times that of Arago.

Secchi has made observations on radiant heat to be found in *Le Soleil*,‡ the most important results of which are that the heat diminishes from the center to the edge; that for a given point there is a satisfactory agreement between the absorption of light and heat; and that the equatorial regions are hotter than the polar. The present writer has made somewhat extended researches in the same direction which are chiefly unpublished. Vogel has made interesting researches in the relative actinic absorption.

None of those who have attempted to compute the amount of the solar absorption appear to have drawn conclusions from their results as to its effect on terrestrial temperatures, and yet if the absorption be anything like what has been found by La Place, and by Secchi using La Place's formulæ, the subject deserves attention.

It will be shown in a forthcoming memoir that the absorption is much less than these values, but that definite limits can be assigned in which it must lie, and that at any rate it is of such importance that the conditions of animal life upon this planet largely depend upon it. It will further appear probable that with a slight change in the depth and absorptive power of this atmosphere, fluctuations in terrestrial temperature will ensue, very great in comparison with any actually observed within historic periods, and it will be shown that this atmosphere is not in a strictly stable condition. Though the subject

* Oeuvres Complètes, Tome i, p. 235.
† Mémoires de l' Académie de Cherbourg.
‡ Le Soleil, 2me edition, Paris. 1875.

then is a nearly neglected one, and the methods of investigation of less apparent interest than those of the spectroscopist, the results are, if verified, of a peculiar interest, from their bearing upon the possible changes in the mean temperature and climatic conditions of our own planet.

The portion of this atmosphere chiefly concerned in absorption I have been led to believe from several considerations is extremely thin, and I am inclined to think it is nearly identical with the "reversing layer" at the base of the chromosphere observed by Secchi and Young, though the other chromospheric strata doubtless, have some share in the obscuration made, and, in a still less degree, the other solar envelopes.

The methods used for heat measurements at Allegheny have been partially described,* and need not be enlarged upon here. Those for light are believed to be novel in their present application, and may be given briefly.

We have seen that a photometric comparison of the center and edge of the sun is for some reason attended with risk of error which we should not anticipate, but which must be great from the enormous discrepancies existing among practiced observers; for, taking Arago's value as the standard, Liais' is four hundred per cent, and Secchi's three thousand per cent greater. Now skilled observers, in comparing their estimates of the excess of the light of a gas flame over that of a candle, for instance, by the familiar methods of ordinary photometry, need not be expected, as common experience shows, to commit errors which are in any way comparable to these.

If such familiar means as the Rumford and Bunsen photometers could be used for the *direct* comparison of the solar intensities there seems no reason why the results should not be of the exactness obtainable in the physical laboratory, or at least of the same order of accuracy. If results can be obtained by such means, they may at least be hoped to be free from gross error.

At first sight it appears that these methods are not applicable to the direct comparison, for the ordinary use of either photometer supposes that the relative distances of the lights from the screen can be altered, while we cannot change the relative distances of the lights to be here compared, which are two portions of the sun; and, if we attempt to enfeeble the stronger lights by shades, or by diffusing it through a lens till it equals that of the other seen direct, we lose the peculiar advantages of the methods which assume that the lights are viewed under the same conditions and measured by the relative distances from the screen.

I arranged, in June, 1874, an apparatus which appears to be free from these objections, and which is here described in

* Comptes Rendus, May 22, 1875.

principle, not in detail. Let us suppose that to the equatorial is attached a screen upon which is described a large circle whose radius is divided into 100 equal parts, and whose center is always in the prolongation of the optical axis. This screen receives an enlarged solar image from an amplifying lens. To fix our ideas we may suppose that the circle and image are each always 24 inches in diameter, and that a direct comparison is to be made of the light of the center with that of a point $\frac{3}{4}$ of the way from the center to the western edge of the sun. Two small* and equal circular apertures are made in the screen at points equidistant from the center or 75 divisions of the scale apart. The telescope is directed to neither point under examination, but to a point of the solar disc $\frac{3}{8}$ of the way to the western edge from the center, and which is therefore midway between the points to be measured. The image of the center now falls on one of the small apertures in the screen; that of the point under examination on the other, and through the apertures pass two cones of rays each slightly divergent, formed under precisely the same conditions, since they come from the same lens and are equidistant from the optical axis. Two prisms of total reflection (cut from the same piece of glass) are placed one behind each aperture, and these deflect the rays toward each other, so that the reflected portion of the cones have a common axis behind and parallel to the screen, within a chamber which is lined with black and shielded from light, so as to form in fact a camera obscura. There is a small Bunsen disc behind the screen and in the camera, whose center remains in the axis common to the reflected cones as it (the disc) slides back and forth on a graduated scale which measures the distance from the source of illumination. Evidently one light is diffused and the other concentrated by the disc's advance and recession upon the cones, sensibly as though these were formed by lights at their virtual apices beneath the screen, and by lights which were of the color and intensity of the portions of the sun under examination. In practice it is found convenient to introduce a lens of short focus between each aperture and its reflector, so that the cones, while still exactly similar, may be less acute. By the substitution for the Bunsen disc, of a small box, sliding also on the graduated scale, and containing a rod whose shadows are cast, by means of a second pair of reflecting prisms, on a prepared surface, sliding with the box, we convert the instrument into a Rumford Photometer, and in this form also it has been much used here. This application incidentally permits under very favorable circumstances a comparison of the *color* of the light from different parts of the sun, which is by no means uniform. When light

* The diameter of the aperture is on the above scale about 0′·10″.

from near the edge is juxtaposed with that from the center, the shadow illuminated by the former is chocolate-red,* while that from the latter is a peculiar bluish tint, nearly like that given by the shade-glass called by English opticians "London Smoke." (I use these comparisons not being able readily to assimilate these colors to any of the pure spectrum.) It is worth while to remark that this observation confirms others obtained from different means by the writer who has elsewhere announced the fact of a selective absorption in the solar atmosphere of such a nature that were the envelope removed the gain in light would be greater than the gain in heat. The light we receive from the sun and universally call "white" is thus seen to be, upon the whole, less refrangible than that which the sun would emit if deprived of its atmosphere; in which case it is evident that the color of our luminary as it grew brighter would tend toward *blue*. If then the depth of its atmosphere were sufficiently increased, our sun, in growing darker, would also appear more *red*. To more than suggest the possible influence of such a cause on the colored stars, among which the bluish are distinguished by the absence of absorption spectra, and the desirability of ascertaining whether change in color, if any such are verifiable, are or are not accompanied by slight changes in apparent magnitude, would lead us away from the present subject.

So clear an exhibition of color in the actual comparison is a sign of the delicacy of the Rumford method, but it introduces a disturbance in our estimation of the intensities of colored lights. An arrangement of the Masson photometer (in which black and white sectors on a rapidly revolving disc illuminated by the colored lights are viewed by the intermittent electric discharge) has been prepared to obviate this; but the complete examination is intended to include that of juxtaposed spectra formed from the lights under examination, selected portions of which of different wave lengths will be compared, as regards intensity, by processes which are a development of those mentioned above. In all such comparisons the apparatus is reversible, so as to eliminate any inequalities in the material, position, or condition of the rays under examination due to the instrument; and in practice corrections for every form of instrumental error are applied which are not here mentioned.

The methods above indicated are, with some slight modifications, evidently adapted to the comparison of the light of the sun-spots, and the light adjacent to the limb, with that of the center; to studying the rate of diminution of light without the disc, and to like investigations; and they are now being so used.

* This color appears to have passed unnoticed by all observers but Secchi, who remarks that the edge of the disc is smoky-red.

Without entering into detail, I may observe that the measurements of the light of spots, though as yet incomplete, warrant me in stating that the absorption of the more refrangible rays, though great in reference to the less refrangible, is not so great as would appear from published measurements. It has long been supposed that the umbræ of spots were not absolutely dark, while it has been admitted, from Dawes's observation, that there is within the umbra a sensibly black "neucleus," and from the assumed blackness of this "neucleus," several astronomers have been led to suppose that the "neuclei" are not openings into a dark gaseous interior.

The measurement of umbræ, and of so-called "neuclei" here shows not only that neither are absolutely dark, but that the absolute light of either is enormous, that of the average "neucleus," so-called, being, as I find, at least *five thousand times* that of the full moon.

La Place, in assuming that the radiation in any direction is constant, and is proportional to the radiating area (or that the radiation is infinite at the edge of the disc), concludes that the total absorption is represented by the integral

$$2\pi\int_0^1 \varepsilon^{-\frac{1}{\cos\theta}} \sin\theta d\theta$$

where θ is the heliocentric angle between the earth and the point of the solar surface under examination; and this assumption has been adopted by Father Secchi, who, in his latest edition of Le Soleil, gives results derived from the use of this integral. I shall, however, assume, in accordance with what seem to be the teachings of modern physics, that a globe as large as the sun whose photosphere, though composed perhaps of very light vaporous material, is yet opake at a limited depth, (as observations on superposition of cloud-strata have shown) that in such a globe radiation would be proportional to the cosine of the angle between the normal to the surface at any point and a line drawn from that point to the observer's position; or, that the sun, deprived of its atmosphere, would appear as a flat disc; whence it will follow that, the total radiation of the sun without the atmosphere being unity, that which escapes through the atmosphere will be, if we adopt La Place's formula with this correction, $2\pi\int_0^1 \varepsilon^{-\frac{1}{\cos\theta}} \cos\theta\sin\theta d\theta$.

These expressions, if La Place's assumptions are otherwise correct, should give, for any comparison of the heat at a given point of the disc to that of the center, a certain value of the absorption which should be constant for any value of θ. In fact I find, however, that this is not the case. The discrepan-

cies in the summation I obtain for different values are not however casual and irregular, as they would be if due to errors of observation, but are systematic. This formula even as here modified, though analytically speaking correct, yet appears to rest on assumptions which are in disaccordance with facts ascertained since La Place wrote. These facts, as they seem to me, demanding a changed formula I expect to point out in a definite form in a subsequent memoir. It seems fitter then to defer an exact statement of the limits of the amount by which solar radiation is absorbed by the atmosphere, until it is accompanied by demonstration. I will only here repeat that we may feel certain that the estimates cited from La Place and Secchi, and which make the sun's atmosphere absorb from $\frac{7}{8}$ to $\frac{1}{1}\frac{1}{2}$ of the radiation we should receive in its absence, are in excess of the truth. For the sake of indicating approximately the real value, I may also state that from my computation of the so-called "luminous-heat" rays, not greatly less or more than one-half the whole are absorbed by, turned back by, or converted into work in, the sun's atmosphere. The total thermal absorption is somewhat less than that of the luminous heat. If, however, we admit this value provisionally, certain results seem to follow which deserve mention.

The mean surface temperature of our globe is separated from that of absolute zero by about 500° of the Fahrenheit scale. The internal heat supplied to the surface may be neglected. The temperature of interplanetary space is, according to Fourier −60°C., according to Pouillet −142, according to Liais −97. Liais, in a memoir whence we cite these estimates, admits, as a consequence of his own, that the obscure heat received at the upper limb of our atmosphere from space (that is, from the chiefly non-luminous matter which occupies it) is greater than that furnished directly from the sun, a conclusion which we are not called upon here to adopt, further than to observe that with this, as with either of the other estimates, the obscure heat received from interplanetary matter by reflection from the sun at the surface of our atmosphere is considerable, so that, if our luminary were wholly extinguished, the temperature of the earth would fall much below that it would reach if only the direct solar heat were withdrawn.

We shall perhaps be warranted in entertaining it then as a reasonable assumption, that, in the complete absence of the sun, the earth's temperature would fall very nearly to −273° C. We prefer as the basis of an estimate, confessedly but approximate, to take the mean of this value and Pouillet's, and, using the Fahrenheit scale, we may state that of the 500° F., which on the natural scale is the approximate mean temperature of our globe, as much as *four-fifths* is derived from the sun.

If it be true then that the sun is surrounded by an atmosphere whose principal action in obscuring the heat radiation is due to a thin stratum which cuts off one-half of the heat which should reach us, and in whose absence this radiation should be doubled,—an atmosphere not independent of the interior of its globe in such a degree as our own, but one to and from which matter is constantly being added and withdrawn, it follows that any change in the ratio of supply and withdrawal, or other cause, which should increase its absorption by so much as 25 per cent would diminish the mean surface temperature of our globe by 100° F., whilst a like diminution in the envelope would produce a corresponding change in the opposite direction.

I am unable to see that, if the Allegheny observations are correct, whence I have derived the above given approximate value of the absorption, any other result would follow, and in any case the existence of life in its present forms on this planet seems dependent, within certain limits, on the depth and absorptive power of the solar atmosphere. The reader who may not have closely observed how far the validity of this result is dependent on, and how far independent of, the hypothesis just made as to the mean terrestrial temperature, is invited to remark that there is nothing hypothetical in any case in the assertion that the earth would fall, in the absence of the sun, to a temperature as low as any actually observed on its surface. If any value attach to the writer's measurements, then, it will be found from a repetition of the calculations with (arctic) temperatures *actually observed*, and hence within the truth, that the result remains that a comparatively slight thickening of the solar atmosphere, or an increment of its absorptive power by as little as $\frac{1}{4}$, would bring the planet back to what we must suppose were the temperatures of the glacial epochs.

Father Secchi seems to think it probable that the difference between the measurements of the heat at different portions of the solar disc made at Rome in 1852 and at Allegheny in 1873–4 are explicable by a real change in the atmosphere of our luminary. To the writer it seems that such a change as Father Secchi assumes, if real, is (if interpreted by Father Secchi's own formulæ) likely to have already altered the mean annual temperature of the earth to an amount sensible to every inhabitant of its surface. He is himself, therefore, of the opinion expressed by M. Faye that no evidence of real variation has yet been established.

It will not of course follow that changes may not take place in cycles of time long with reference to historical periods. All analogy leads us indeed to think an absolute uniformity most improbable.

Finally, then, it appears that though we are justified, on grounds established by Helmholtz and Ericsson, in believing in the constancy of the heat-supply *within* the solar envelope for periods to come which are with reference to our terrestrial past almost infinite, we are not justified by our present investigations in asserting that the solar radiation has been constant during geologic periods, or is certain, or even likely to remain, what we now see it during corresponding periods in the future.

If there be great cyclical changes of long period (and in our present ignorance we can only say that such are not antecedently improbable) there will be corresponding changes in terrestrial temperature; and it is allowable to inquire whether we do not find here matter for consideration in connection with those great changes of temperature in past epochs which have in them nothing hypothetical, which geology assures us have indeed existed, and for whose possible cause no satisfactory suggestion has hitherto been made.

ART. LX.—*On Southern New England during the melting of the Great Glacier.—Supplement: The Overflows of the flooded Connecticut;* by JAMES D. DANA.

IN the remarks on the occurrence of Reindeer remains in the vicinity of New Haven, on page 356 of this volume, I have stated that the reindeers must have been living in the Quinnipiac valley after the retreat of the glacier and before the glacial flood; and on page 425 it is shown that the New Haven rivers must have had their floods from the melting glacier greatly prolonged by accessions derived from overflows of the Connecticut. It is evident that such overflows would have had an important influence on the geology of Southern New England, and I therefore present here the evidence I have been able to collect with regard to their positions and courses.

But, by way of introduction, I first mention some particulars from the older geological history of the great central valley of New England, since they serve to explain in part the occasion of these phenomena in its later history.

I. THE CONNECTICUT VALLEY BEFORE THE GLACIAL FLOOD.

It is well known that, in the Triassic period, and probably also the Jurassic, the Connecticut valley, from New Haven to Northern Massachusetts, was occupied by a brackish-water estuary—its length over 110 miles, its width for the most of the way 20 miles or more. Farther north, the valley for a long distance is narrow, and here the waters were probably those of a true

river. The sides and bottom of the estuary were of crystalline rocks, similar to those outcropping over the bordering regions. The sand-flats and mud-beds that were made in the great estuary, out of the material contributed from either side by the rivers of the era, constitute its Red-sandstone formation.

The red sandstone of the area is now 300 feet above the sea-level near New Haven, and over 1300 in Massachusetts—a fact showing that, since Jurassic times, New England has been raised over its center at least a thousand feet more than along its southern coast.

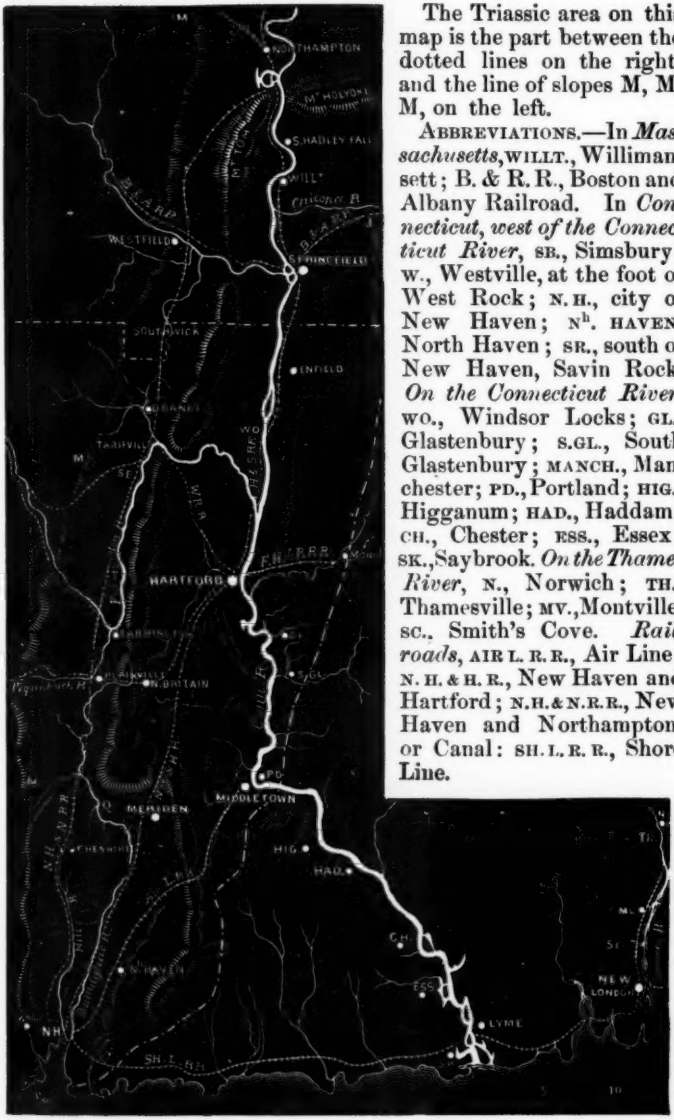
With the exception of the seaward slope thus occasioned, and the erosion by denuding waters, the area of the Triassic sand-beds might have remained till now a wide plain, with the river of the valley flowing through it in a deep channel of its own making, and emptying into the Sound at New Haven, had it not been that the deposition of the beds was followed, over the whole area, by subterranean movements that ended in tilting them, making numberless long fractures to unknown depths, and filling the fissures so opened with melted rock. The sandstone was by this means left intersected by dikes of trap, along with adjoining hard-baked portions of its own beds which could stand wear a hundred fold better than other parts of the formation; and, as erosion went forward, the trap dikes became the trap ridges of the country, standing, with some of the enclosing indurated sandstone, as high barriers between different parts of the sandstone area.

A prominent line of these trap dikes divides the area, in a north-and-south direction, from Mount Tom (1214 feet high) near Northampton, Mass., to West Mountain (996 feet high) three miles northwest of Meriden; the valley is thus divided into an *eastern* and a *western* section, the former three times the widest. This range is called in the following pages, the Mount Tom Range, and also Divide Range.* This range is continued south of the Meriden hills in a low ridge of sandstone to Mount Carmel (736 feet high); then, by another lower ridge of sandstone to East Rock (360 feet high) nearly abreast of New Haven city.

For the convenience of the reader of this and other recent papers on New England geology, by the writer, a map of the

* This line of ridges from New Haven to Northampton was called the Mount Tom Range by President Timothy Dwight in 1796 (Travels, p. 5). Looking only at the general features of the country, he made this range a branch from the White Mountains; and the elevations of Western Connecticut, commencing at the West Rock trap range, near New Haven, he spoke of as a part of the Green Mountains. The error was not a bad one in 1796. But it has been reproduced since in geographies, encyclopedias and gazetteers, and recently it has appeared in a school book of Connecticut origin, and in a Guide book for the Connecticut Valley. It is alluded to here in order if possible to stop its further circulation. Over 50 years since, Professor Silliman corrected the error in this Journal, and yet it still survives.

MAP OF THE CONNECTICUT VALLEY AND PART OF THE COAST OF SOUTHERN NEW ENGLAND.



The Triassic area on this map is the part between the dotted lines on the right, and the line of slopes M, M, M, on the left.

ABBREVIATIONS.—In *Massachusetts*, WILLT., Williamsett; B. & R. R., Boston and Albany Railroad. In *Connecticut, west of the Connecticut River*, SB., Simsbury; W., Westville, at the foot of West Rock; N. H., city of New Haven; N^h. HAVEN, North Haven; SR., south of New Haven, Savin Rock. *On the Connecticut River*, WO., Windsor Locks; GL., Glastenbury; S.GL., South Glastenbury; MANCH., Manchester; PD., Portland; HIG., Higganum; HAD., Haddam; CH., Chester; ESS., Essex; SK., Saybrook. *On the Thames River*, N., Norwich; TH., Thamesville; MV., Montville; SC., Smith's Cove. *Railroads*, AIR L. R. R., Air Line; N. H. & H. R., New Haven and Hartford; N.H. & N.R.R., New Haven and Northampton, or Canal: SH. L. R. R., Shore Line.

Connecticut valley and part of Southern New England is here annexed.

These trap barriers naturally determined to a large extent the drainage-lines of the area. Since the eastern section is three times the widest, and the freest from dikes of the fire-made rock (trap), it is not strange that it became the course of the Connecticut River. But south of Hartford, between that city and Meriden, a succession of trap ridges rises to the east of this range, which crowd toward the river; and, consequently, the Connecticut, after passing Hartford, loses its westing; and then, at Middletown, where the metamorphic rocks are near in high elevations, turns abruptly out of its old valley through an opening heading southeast that then offered no doubt an unobstructed way to the Sound. Thus the southern part of the valley lost the Connecticut River. This event in New England history occurred before the Cretaceous period. After this, geology has no special facts from the valley until the Glacial period.

The western section of the sandstone area, shows that it was too contracted to become the course of the Connecticut by its subdivision into many river basins. The Northampton region is divided from the Westfield, and the Westfield from the Farmington. Moreover, the Farmington, while essentially one valley in its plains from Granby to New Haven, fifty miles, has three prominent rivers in this distance. Farmington river, the largest of them, makes a long V-shaped bend in the valley, first south and then north (as the map on p. 499 shows), and finally flows off into the Connecticut valley through a gap in the Divide Range near Tarifville. The eastern arm of the V lies in the broad flat meadows of the valley, with no ledge or hill to prevent the river's pushing southward instead of northward. In that same low plain, not two miles distant from the south end of the Farmington V, the Quinnipiac river becomes the river of the valley; but after flowing on for ten miles, this stream bends eastward out of it to the Meriden region, and then goes again southward, taking the way toward New Haven Bay that the Connecticut would have followed had it not been diverted southeastward at Middletown. Finally, where the Quinnipiac is deserting the valley, Mill River, the third of the streams, commences within the low Quinnipiac plain; and this stream holds its place in the valley to New Haven.

The facts here stated should be reviewed on the map in order that the remarks beyond, on the drainage in the time of the great flood, may be appreciated.

The Glacial period was one of extensive denudation throughout the valley, the ice working along with the subglacial waters; and the waters had unwonted denuding power, if the land was

of higher level than now with the elevation increasing northward. The red sandstone of the valley is a soft rock, yielding easily to abrading agents; and through its removal the trap ridges of the valley may then have derived the larger part of their prominence. From the height above the Connecticut flats of the sandstone ridge called Mount Toby (or Mettawampe) in Sunderland, about thirteen miles north-northeast of Northampton, we learn that not less than 1200 feet of rock in depth has been removed from the sandstone formation of that part of the valley since the Jurassic period, of which a large part was probably the work of the Glacial era; and, on the same kind of evidence, that the removal near New Haven has exceeded 300 feet.

II. The Connecticut Valley during the Glacial Flood.

In the next or Champlain period, when the land had subsided, and most deeply so to the northward, and when the melting of the glacier had far advanced, the valley was put under new conditions, as has been already explained; and, according to the evidence mentioned beyond, New Haven Bay was restored in part to its old rights by overflows giving it some of the surplus waters of the flooded Connecticut.

The height of the flood from the melting glacier as illustrated in the last number of this Journal, from the height of extensive terraces of stratified drift—was such that, at Middletown, the waters, instead of stopping at 24 or 25 feet above low-water mark, as now, continued rising until 175 feet deep; and from that level, they went plunging down the narrow channel to the Sound; that, at Hartford, the river rose above the modern 30-foot flood-level to 190 feet; that, at Springfield, they reached a height of more than 200 feet above low-water mark, and 240 feet above mean sea-level.

It admits of proof, further, that when the flood was at its height, the Connecticut overflowed in at least three places, westward, into the western section of the valley, and there flowed southward to New Haven Bay. These three places are—

First, over the Meriden divide, as has been already explained.

Second, at Westfield, west of Springfield, whence the waters descended along the western section of the valley, and finally joined Quinnipiac and Mill Rivers—New Haven streams.

Third, from the Northampton region, by the valley west of Mount Tom, over the Westfield divide to Westfield, there to join the Westfield overflow.

As to other westward overflows to the north I do not speak, as I have made there no special examinations.

The fact and the features of the western section of the val-

ley have been long appreciated. So early as 1822 it was selected as the route for a canal from New Haven to Northampton, to be named the Farmington canal, which by 1835 was finished; and in 1847 the route of the unprofitable canal was taken for a railroad, called the New Haven & Northampton, or Canal, Railroad. Prof. Hitchcock, after an examination of a map and section of the canal, queried why the Connecticut river had not made for itself a channel along that route, rather than the more eastern; and in his "Surface Geology" he describes that section from Northampton to New Haven as a branch of his "second basin" of the Connecticut valley.

On page 140 of Prof. Hitchcock's first Geological Report of Massachusetts (8vo, 1835), and p. 329 of the second edition (4to, 1841), he has the following remark: "In tracing the Connecticut through this [its] valley the geologist will be surprised to find it crossing the greenstone ridge above described, and that too at its highest part, viz: through the gorge between Holyoke and Tom. For he will naturally inquire: Why did not the river flow through the valley *west* of the ridge, and, following the course of the Farmington Canal, empty at New Haven? for it appears from the surveys on this canal, that in no place is that portion of the valley more than 134 feet * above the present level of the Connecticut at Northampton, whereas the trap ridge through which it passes is from 800 to 1,000 feet high.

The denudation required for "crossing the greenstone [trap] ridge" appears less difficult when the fact is recognized that Mt. Tom and Mt. Holyoke are two independent trap-dikes of very different trends, the latter an east-and-west dike; and that, as is seen at the eastern foot of Mt. Tom, along the river, there was sandstone between them; so that it was sandstone that was removed for the river or by the waters of the valley, and not trap.†

Prof. Hitchcock says, in his Surface Geology (1857), speaking of the topographical basins into which the Connecticut Valley may be divided (p. 11): "The second basin extends from Hol-

* The survey for the Canal found the greatest height two miles north of Westfield, between Westfield and Northampton, and made the height of the top of the divide above the level at Northampton, 86 feet, and the height of the latter above the Connecticut River 48 feet; and 86+48=134.

† Dr. Percival, in his Report on the Geology of Connecticut, brought out the fact that the trap ridges of the Triassic area are often made up of two or more trap-dikes separated by sandstone; that is, are formed from the fillings of two or more independent fractures—independent at least at surface. Pine Rock, a trap ridge east-northeast in trend (like Mt. Holyoke) in the New Haven region (P on the map of p. 415), appears as if it were a continuation of the south end of West Rock, which turns toward it, and still they are wholly distinct; and Pine Rock, although not half a mile long, is actually a combination of four masses or dikes of trap with sandstone between, and a third of the length is sandstone. Again, East Rock (E, on the same map) consists of three dikes with intervening sandstone. It is owing to this composite character that gaps through the high trap-ridges are not uncommon. The associated sandstone, while generally hard-baked, is sometimes cracked into small chips as a consequence apparently of too *dry* heat in the baking.

yoke to Mettawampe (Mt. Toby) in Sunderland and Sugarloaf in Deerfield. From Holyoke this basin must have extended southerly along the *west* side of Mt. Tom and the other trap ranges that extend almost continuously to New Haven. Through this valley runs the Canal Railroad, but nowhere is this valley more than 134 feet above the Connecticut at Northampton, and this is not so high as some of the terraces."

The idea here conveyed is expressed also on the map of the Connecticut Valley, forming plate III, of the same volume. A terrace-plain, numbered 2, is made to spread southward from Northampton, or rather from Greenfield more than twenty miles north of Northampton, through Southampton, Westfield and Southwick in Massachusetts, and Granby in Connecticut. The map, however, presents a suggestion of the author's mind rather than a fact at the time established: for terrace No. 2 of Northampton, the highest mentioned for that place, is according to the text, only 97 feet above low water in the river instead of 134 feet, which makes it nearly 40 feet too low to pass over the divide to Westfield.*

Further, it is necessary to note, in order to apprehend Prof. Hitchcock's views, that in discussing the origin of terraces in the same work (p. 49 and beyond) he makes the ocean a chief agent in producing "beaches and terraces," and supposes a submergence of the land of 2,000 to 2,500 feet (pp. 53, 54), attributing part of the effects to icebergs, marine erosion, and the making of seashore beaches and flats during the period of submergence and during a progressing emergence, and part to the drainage which was again set in action by the emergence. In his Geological Report (1841) he makes the terraces a result of river erosion. The terraces of the Connecticut Valley, in the view I have presented, are wholly of fluvial origin, and the highest terraces of the various valleys, whether over the lower or higher parts of the country, are approximately measures of the flood-level in each.

The proof with regard to these westward overflows is derived from the terraces of stratified drift over the regions.

1. *Overflow at the Meriden divide.*—On this overflow it is necessary here only to repeat in brief the facts from page 425

* The numbers affixed to terraces by Prof. Hitchcock, in his Surface Geology, express the succession at a locality, beginning with the lowest as No. 1: so that terraces of different localities that are the same in height have often different numbers. The Northampton region has only two terraces, numbered 1 and 2, respectively 57 and 97 feet above low water in the Connecticut, and the latter stated to be 202 feet above the sea-level. At Whately, half a dozen miles north, there are three, Nos. 1, 2, 3, respectively 32, 46 and 92 feet in height, the last 206 feet above the sea-level, and therefore the same with No. 2 of Northampton; yet this No. 3 has a different color on the colored map. No. 2 at Greenfield, a dozen miles farther north, is 291 feet above the sea-level, and therefore different from either of those at Whately or Northampton. This feature of the work has to be noted in order rightly to apprehend the facts it presents.

Prof. Hitchcock also has "moraine terraces" and "sea beaches" among the phenomena of high levels over the country—which I have been led to regard as mostly terrace-formations at high levels where there were water-courses during the era of the melting glacier, if not so now.

of this volume: that the divide 1½ to 2 miles north of Meriden, is on the borders of the Hartford river-region: that the highest part of the divide is near the Hartford flood-level, about 160 feet above modern high water in the river; and that over 20 feet of terrace sands and gravel, that is, stratified drift, underlies the summit plain. The red sandstone outcrops in some places over the top and rises into ledges. The overflowing waters eroded the sandstone surface, and descended to the Quinnipiac, just below Meriden, to swell the stream and increase the height of its terrace depositions.

2. *Overflow from Westfield, Mass., over the Southwick divide, into the Farmington Valley.*—The village of Westfield is situated nine miles west of Springfield, on Westfield River, a large and rapidly flowing stream tributary to the Connecticut. The river emerges from its confined valley of metamorphic rocks, about twelve miles west of the Connecticut River, at "Mt. Tekoa," and there enters the open Red-sandstone area in which Westfield is situated. Five to six miles west of the Connecticut, it passes through a gap in the Mt. Tom or Divide Range, and enters the present Connecticut Valley. The Westfield region is hemmed in on the north by the Westfield divide, between it and Northampton, and on the south by the Southwick divide, separating it from the Farmington Valley—both made of red sandstone, but having, over the sandstone, levelled deposits of stratified drift.

The upper terrace of the region extends from the city of Westfield three miles westward.* The material is sand and

* See a colored map of the Westfield terraces in Hitchcock's Surface Geology, Plate VII. The author expresses his doubts with regard to the heights he gives. His No. 4 corresponds nearly with the lower part of the upper terrace as measured by Mr. Dunham. The region along the course of the old Farmington canal, from New Haven to Northampton, was surveyed for the canal-route by Mr. H. Farnam, then Assistant Engineer, and in charge of the work. The section published on a map engraved by Messrs. N. & S. S. Jocelyn, of New Haven, in 1828, gives all the lockage to Northampton, and, in fact, beyond this to White River Junction, its projected termination. The section makes the water level of the canal over the Southwick divide, through the Southwick Ponds, 220 feet above mean sea-level; at Westfield, alongside of the present Railroad Station, and nearly on a level with the track, 144·75 feet; over the Westfield divide, 230·75 feet; at Northampton, 48 feet above its termination at water-level in the Connecticut, 144·75 feet. Mr. Farnam informs me that the plain of the Southwick divide was as I had estimated, about 20 feet above the level of the Ponds, and thence about 240 feet above mean sea-level; that of the Westfield divide, about 10 feet above the level of the canal, or 241 feet. As bearing on these levels I state further that according to the surveys of the Connecticut River, under Gen. T. G. Ellis, the height of the railroad track at the Springfield Station is about 70·4 feet above mean sea-level. Mr. H. F. Dunham, assistant in the office of the City Engineer, Springfield, obtained, in a careful levelling from Springfield (obligingly made for this place), 148·84 feet for the level of the track (top of rail) at the Westfield Station. He judged that the water-surface of the canal was probably 2½ feet below the same level. By adding 53 feet (from R. R. levelling) to 70·4 gives 123·4 for the height of the track at the Northampton station. But according to Mr. E. C. Davis, Civil Engineer in Northampton, the track there is about 19·7 feet below the level of the old canal.

gravel, as usual—with the gravel, at the section I examined in the city, constituting the upper 25 feet, excepting 2 or 3 feet of sand at top. According to a levelling by Mr. H. F. Dunham (assistant to Mr. G. A. Ellis, of Springfield,) the height near its eastern limit is about 239 feet above mean sea-level; and from this it gradually rises westward—or up stream—to 286½ feet. The 239-foot level is very near that of the highest flood about Springfield (240–245 feet); it is about 96 feet above modern flood-level in Westfield River.

The proof that the waters of the flood passed the Southwick divide into the Farmington Valley is as follows:

1. The upper terrace plain of the divide is not above the height of the upper terrace plain of the Westfield region.

2. The Southwick Ponds, which lie along the lowest part of the divide, where it was passed by the Farmington Canal, are lower than the lower level of the upper Westfield plain.

3. The terrrace plain of the divide may be followed down the Farmington Valley.

On the 1st and 2d of these points I observe that the height of the Southwick Ponds is about 78 feet above high water in Westfield River, or 220 feet above mean high tide—which is nearly 20 feet below the lower part of the upper Westfield terrace. A terrace bordering the ponds carries the height up to about 240 feet. The level rises to the northwest over the Southwick plain: but there, along the railroad track, the height is but 260 feet, which is 25 lower than the higher part of the Westfield terrace.

We may hence conclude that before the flood exceeded in height 220 feet, the flow southward had begun; and that as the waters rose the terrace deposits above that level were laid down over the divide. Since 240 feet was also the height of the highest terrace on the Connecticut east of Westfield, the waters of the overflow may have been in part those of the Connecticut; but it is probable that they were solely from Westfield River—waters which the Connecticut was thus deprived of; for the height of the flood over the middle of the Westfield basin was at least 275 feet, as shown below, the waters having been held up to this level (30 feet above their height at Springfield) in consequence, probably, of the narrow passage for them below Westfield through the Divide Range.

As to the third point, I give the following table of heights of the upper terrace along the valley south of the divide—the terrace plain in each case, the first excepted, being extensive.*

* The terrace at Tarifville rises at first abrubtly, near the railroad station, and afterward with a slope which extends beyond the cemetery; the deposits are of fine earth, excepting the upper 20 feet which are pebbly. The top is not clearly defined, but appears to be indicated by the pebbly beds. The Simsbury terrace is an elevated plain, directly west of the railroad. There is an extensive plain about

	Distance south of the Divide.	Height above flood-level.	Height above mean sea-level.
Tarifville	8½ miles.	115 feet.	275 feet.
Simsbury	12 "	110 "	270 "
Farmington Station	22 "	80 "	254 "
Plainville	26 "	49 "	234 "
Southington	30½ "	75 "	223 "

The facts show that the flood had a height over the Southwick divide of at least 270 feet; that this level was kept up as far south as Tarifville and Simsbury by the inflow of the flooded Farmington; that south of Simsbury the decline in height was very gradual; and that even at Plainville, where the flat valley spreads to a great width, the terrace was made to a height but little lower than the plain at the divide. At Tarifville, the Farmington is reached and here the waters from Westfield joined those of that river.

The high level at Tarifville and Simsbury is very remarkable, considering the open cut through the Divide Range by which the stream now enters the Connecticut valley, and the fact that the terrace-plain east of the range is full 50 feet lower than that west. But the cut is not 100 yards wide; and, besides, it may have been filled with drift from the glacier (as was the Niagara channel), and the removal of the obstruction have not begun until the flood had reached its height.

Reaching Plainville, the waters left the present Farmington River area, to enter the Quinnipiac; for there was nothing to prevent, all being one plain below; and a Farmington valley terrace 49 feet high continues southward as the terrace of the Quinnipiac. Thence they went down the Quinnipiac Valley to New Haven Bay.

Where the Quinnipiac River, ten miles from its source, below Southington, commences to bend out of the valley, part of the waters joined Mill River—no impediment existing there in hills or ridges; and the upper terrace of the Quinnipiac continuing on down Mill River.

Thus the flooded Farmington, swollen still farther by waters from the Westfield overflow, occupied the whole Farmington Valley from the Southwick divide to the Sound; and the flood discharged into New Haven Bay by two of its streams, Quinnipiac and Mill Rivers.

It is a misfortune to the State of Connecticut that the Farmington River did not take advantage of the opportunity thus afforded to dig a channel deep enough to ensure its permanent flow to New Haven; since, with such a river, the bay would have made one of the best harbors on the New England coast.

the Granby Station at about 210 feet above mean sea-level. The terrace measured at Plainville is on the east side of the Farmington Valley, 1½ m. from the railroad station, close by the Quinnipiac. I am indebted to Mr. H. F. Root, Civil Engineer on the Canal Railroad, for heights along the road.

3. *Overflow from the region of Northampton, by the west side of Mount Tom, to Westfield, to join the flow down the Farmington Valley.*—The obstacle here was the divide north of Westfield.

The upper terrace of the Northampton region is one of the best defined terrace-plains of the Connecticut valley. It is only two miles west of Northampton, and is known as the Florence plain, part of it being the site of a village of that name. Its height above mean sea-level, according to the survey for supplying the city with water, as I am informed by Mr. E. C. Davis, Civil Engineer, is 260 to 265 feet. The terrace which Prof. Hitchcock gives as the highest in northern Northampton occurs also just west of the city, though with the surface rising westward; its height he gives as 97 feet above low water, and 202 above tide level; and it is therefore 60 feet below the level of the Florence plain.

The Florence-plain level, 260–265 feet, is evidently a northward continuation of the upper Springfield level, that of 240–245 feet. There is hence a difference of level of 20 feet in a distance of 16 miles. But if the land were depressed, with the depression increasing northward at the rate of a foot a mile, the pitch would have been slight. How far the narrows between Holyoke and Tom affected the height of flood-level above and below, I have not investigated.

From the height of the upper terrace-plain at Northampton, we know that the flood-height there was *not below* 265 feet; and probably it was 10 or 15 feet above this, as Hitchcock mentions a terrace of 289 feet near Hadley. What then was the level of the Westfield divide?

This divide at its lowest part, where it was crossed by the Farmington canal, has a height of but 241 feet above mean sea-level, which is more than 20 feet below the terrace-level at Northampton. We have good evidence, therefore, that the overflow took place, and that the stratified drift of the divide owes to it its deposition. High terraces exist along the sides of the valley between the divide and Northampton.

Passing the divide the waters joined those of the flooded Westfield River; and if so, they became part of the overflow which descended by the Farmington, Quinnipiac and Mill River valleys to New Haven and the Sound.

Conclusions.—(1.) The Connecticut when the Glacial flood was at its height had a depth of 150 feet, or more, all the way from Middletown to Turner's Falls at Springfield—and to an undetermined distance beyond; and from Hartford to Turner's Falls it averaged fifteen miles in width. It was a great stream, fed by numerous headlong torrents from either side; and, at the same time, feeding other streams from its surplus waters. Its depth and extent was in spite of great losses from overflows into other valleys.

(2.) The violence of the flood in the Connecticut valley was confined mostly to the time of its maximum height. At Hartford, Springfield, Westfield, and elsewhere, the coarse deposits have been found to be those of the upper portion of the terraces; not of the upper terrace alone, but often also of the lower, and because the low may be of flood-origin as well as the highest. (This volume, p. 178.) When the Connecticut flood at Springfield was about 120 feet above modern flood-level, or 180 feet above mean sea-level, and at corresponding height northward to Northampton, the waters were sluggish; for clay beds, which could be formed only in sluggish waters, are common through the region up to this level; and with the clays, except at points remote from the river, there are only fine sands—other evidence of the absence of all violence of movement. For the next 20 feet the beds at Springfield continue to be of sand. The cause of such an almost lake-like condition over this region, when the waters were already so high, can be explained only by assumptions, and for the present I let them pass unconsidered. Above the 200-foot level at Springfield, the deposits are generally coarse.

(3.) The facts show that the flood of the New Haven rivers did not cease when the melting glacier had disappeared from their valleys, even if so to their very sources. While the glacier was continuing its retreat to the Massachusetts border, the Farmington River may have been pouring its floods down the Quinnipiac; and during the long retreat through Massachusetts and the States north, both the Quinnipiac and Mill Rivers were swollen with waters from the overflowing Connecticut.

Such conditions have to be taken into consideration in order to study rightly the Quaternary geology of the country, or of any Glacial land. The reindeer bones in the clay beds of the Quinnipiac (this volume, p. 353) indicated, by their position and freedom from wear, that reindeers lived in the valley after the retreat of the glacier and before the glacial flood had reached its height; and we may understand from the above observations how and why this was possible. The conditions of all the rivers of the ice-covered land when at flood height, their depths, widths and overflows, must be worked out and mapped, before the events of the Fluvial or Champlain period in the Earth's history can be fully understood or appreciated.

Correction for page 427.—The narrows below Middletown are 650 feet wide at low water and 800 at extreme high, according to Gen Ellis (Rep. U. S. Engineer Dept. for 1859), who thus explains the height of the modern floods above.

Des Cloizeaux on the optical properties of the Feldspars, p. 480.—The following errors occur in this article. In lines 3 and 20 from foot of page, 19° 27′ for 15° 27′, and in the bottom line, 7° 12′ for 5° 12′.

Watson's Descriptive Geometry.—The work noticed on page 488 is by Wm. Watson.

GENERAL INDEX

OF

VOLS. I—X, THIRD SERIES.

NOTE.—The names of minerals are inserted only under the word MINERAL. The references to articles on Botany, Geology and Zoology, are grouped under these words, but at the same time are in general inserted in their places elsewhere.

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ERRATA.

The Errata of the volumes 1 to 9 will be found severally in each. The following are additional.

Vol. iii, p. 383, line 9, for i. e., read e. g.; line 44, for first, read e. g.

p. 384, lines 7 to 13 should be transposed so as to follow line 36; lines 36 and 52, for i. e., read e. g.

Vol. v, p. 9, line 30, for 65° 50′·3, read 65° 58′·3.

p. 11, line 16, for *cuspidatus*, read *cuspidata*.

p. 15, line 11 of notes, for spine, read spire; line 35 of notes, for *helecina*, read *helicina*.

p. 100, line 18 of notes for *Lyngmanni*, read *Lyungmanni*.

Vol. vi, p. 439, line 14, for *Krogeri*, read *Kroyeri;* line 16, for *Nychia cirrosa*, read *Eunoë Œrstedii;* line 24, for *lucida*, read *nitidulum*.

p. 440, line 4, for *sopotilla*, read *sapotilla;* line 9, for *solidula*, read *solida;* last line of note, for 1870 read 1780.

Vol. vii, p. 39, line 6 from bottom, for *Ophiocnida hispida*, read *Ophiacantha spinulosa*.

p. 131, line 6 from bottom, for *Pontagenia* read *Pantogenia*.

p. 132, line 9, for *Borlasia sp.*, read *Cephalothrix linearis*.

p. 134, line 29, for *Eurosalpinx*, read *Urosalpinx*.

p. 444, line 37, for A. Chanean, read A. Chauveau.

Vol. x, p. 40. line 39, for Cauthouy, read Couthouy.

p. 155, line 42, for E. Sesemann, read C. Seemann.

p. 213, line 26, the statement "16 inches in circumference" applies to the basal portion of the short arms.

p. 352, in figure, transfer the letter *S* to the knife-edge from which the wire basket is suspended.

p. 427, line 11 from top, for 400 *yards*, read 800 *feet at highest modern floods;* 13th line from top, for 500 to 550, read 400 to 450.

p. 480, lines 3 and 20 from foot, for 19° 27′, read 15° 27′; and last line, for 7° 12′, read 5° 12′.

p. 488, line 8 from top, for WALTER, read WATSON.